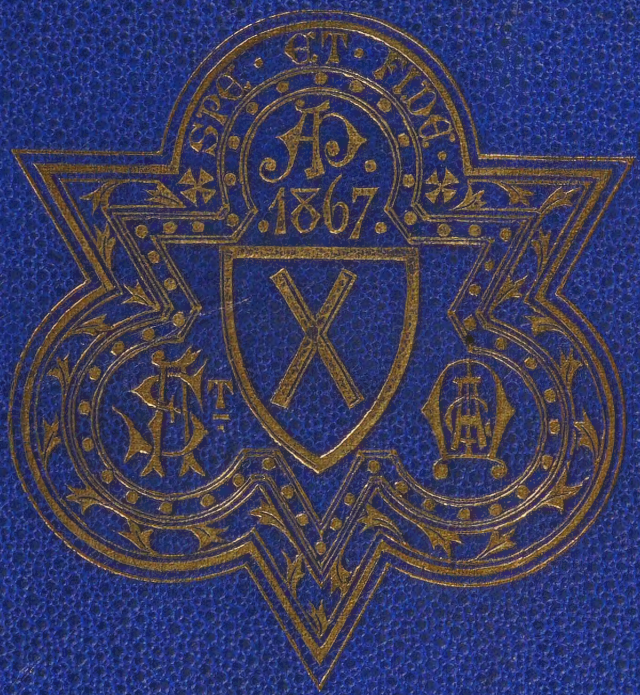
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LAWS

OF THE

ST. ANDREWS MEDICAL GRADUATES' ASSOCIATION.

TITLE.

1.—The Association shall be called "The St. Andrews Medical Graduates' Association."

OBJECTS.

2.—The objects of this Association shall be the advancement of the Science and Art of Medicine, and of General Science and Literature, the maintenance of the interests of the Medical Graduates of the University, and the cultivation of social intercourse and good fellowship.

CONSTITUTION.

3.—The Association shall consist of Members, Honorary Members, and Associates.

4.—All Medical Graduates of the University of St. Andrews shall be eligible as Members.

5.—All Members of the General Council, all Professors, and all non-medical Graduates of the University of St. Andrews, shall be eligible as Honorary Members, as well as such other learned and scientific men as may be recommended by the Council.

6.—All legally qualified Medical Practitioners shall be eligible for admission as Associates.

7.—Members, Honorary Members, and Associates, shall be admitted only at the General Sessions of the Association. The election shall be by ballot, and no one shall be declared elected unless two thirds of the members present vote in his favour.

8.—A Member, Honorary Member, or Associate, may withdraw from the Association by paying such subscriptions as may be due from him, and signifying his intention in writing to the President.

9.—No Member, Honorary Member, or Associate, shall be removed from the Association except in accordance with the following regulations. A written notice of the proposed removal, signed by two members of the Association, shall be sent to the Honorary Secretary, who shall immediately forward a copy of the charge to the member accused, and shall at the same time summon the Council to meet within twenty-one days. He shall send a notice of the subject to be discussed to each Member of the Council at least fourteen days before the date of such meeting. If the Council shall resolve by a majority of those present, that the Member so accused ought to be expelled, a notice shall be forthwith sent to each Member of the Association, making the next General Session special for the consideration of such removal, and if two-thirds of the Members voting, shall be of opinion that the Member in question shall be expelled, the President shall direct the Honorary Secretary to remove his name from the list of Members. The votes shall be taken by ballot.

10.—The subscription constituting a Member or Associate shall be Five Shillings annually, due on the first of January in each year.

EXECUTIVE.

11.—The Officers of the Association shall be elected from the Members, and shall consist of a President, Six Vice-Presidents, a Treasurer, a Secretary, and a Council of Thirty-two; in whom the power of framing bye-laws, and of directing the affairs of the Association, shall be vested.

12.—Five Members of the Council shall form a quorum.

13.—The Officers of the Association shall be elected by ballot at each Anniversary Session of the Association.

14.—The Officers of the Association shall be eligible for re-election, except that two of the Vice-Presidents and eight of the Council shall retire every year.

15.—The business of the President shall be to preside at the Sessions of the Association, and at the Meetings of the Council; in his absence one of the Vice-Presidents, or the Treasurer, or any Member of the Council chosen by the Members present, shall take the chair.

16.—The Treasurer, or some person appointed by him, shall receive all moneys due to the Association.

17.—The money in the hands of the Treasurer, which shall not be immediately required for the uses of the Association, shall be vested in such speedily available securities as shall be approved of by the Council.

18.—The Council shall lay before the Members, at each Anniversary Session, a report of their proceedings during the past year, and also an account of the receipts and expenditure of the Association.

19.—The Council shall meet at least once in two months, unless by special resolution to the contrary.

20.—The annual accounts of the receipts and expenditure of the Association shall be audited by a Committee of three Members, selected at the preceding Anniversary Session from among the Members at large.

21.—The Secretary shall have the management of the general correspondence of the Association, and of such other business as may arise in carrying out its objects.

SESSIONS.

22.—The Association shall hold an Anniversary Session, commencing on St. Andrew's day, or on such other day as the Council may determine. The place of such Session, its duration, and the business to be transacted, shall be arranged by the Council.

23.—The Members and their friends shall hold an Anniversary Dinner on the last day of each Anniversary Session, at such place and time as the Council may determine; the President for the year shall be in the chair.

24.—No alteration in the Laws of the Association shall be made, except at a General Session. Notice of the alteration to be proposed must also have been laid before the Council at least a month previously.

25.—The Council shall have power to call a General Session of the Members at any time, and shall also be required to do so within one month, upon receiving a requisition in writing to that effect from not less than twenty Members of the Association.

26.—All Special General Sessions of the Association shall be held at such place as the Council may appoint.

27.—The Council shall have power to publish the proceedings of the Association, and to make such charge for them as they may deem right.

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1867.

1867.

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London, June 19.

ANNIVERSARY SESSION,

London, Dec. 2nd and 3rd.

GENERAL SESSION, June 19.

A Preliminary meeting for the purpose of constituting the Association having been called by Dr. Richardson at his house on May 29, 1867, a General Session, convened by circular sent to all Doctors of Medicine of St. Andrews in the United Kingdom, was held at the Freemason's Tavern on the 19th of June, when the laws of the Association were agreed to and the Officers for 1867 elected. The President stated the objects of the Association, and a resolution was passed requesting the Council to "urge upon the Government, in such manner as they may deem most expedient, the claims of the Medical Graduates to a vote for the representative in Parliament of the University of St. Andrews.

ANNIVERSARY SESSION, Dec. 2nd and 3rd.

The Session was held at Willis's Rooms, St. James's, and commenced on the 2nd. The minutes of the last Session having been read and confirmed, the Honorary Secretary reported that 402 members and seven Honorary members had joined the Association.

The Treasurer presented his Report.

The Honorary Secretary read the Report of the Council.

The President said that as the address of the Honorary Secretary on the University of St. Andrews and its Medical Graduates, bore very closely on the subjects mentioned in the Council Report, it would be better to adjourn any discussion until Dr. Sedgwick had read his paper, which he then called upon him to do.

Dr. Shorthouse, of Carshalton, moved, and Dr. Waring, of London, seconded,

"That the Report of the Council be received and adopted." Carried unamimously.

Dr. Wynn Williams, of London, proposed, and Dr. Royston, of London, seconded,

"That a cordial vote of thanks be offered to Dr. Sedgwick for his admirable paper, and that he be requested to allow it to be printed in the Transactions." Carried unanimously.

Dr. Wynn Williams, of London, proposed, and Dr. Griffith, of Camberwell, seconded,

"That the Lord Advocate, and if necessary the Right Honourable B. Disraeli, be requested to receive a deputation from St. Andrews Medical Graduates Association, on the subject

of the franchise of the University, and that the members of the Association be requested to join the deputation." Carried unanimously.

Dr. Sedgwick, of London, proposed, and Dr. Wynn Williams, of London, seconded,

"That the law which is numbered VII., be added to the laws of the Association." Carried unanimously.

Dr. Sedgwick, of London, proposed, and Dr. Lloyd Roberts, of Manchester, seconded,

"That the law which is numbered VIII., be added to the laws of the Association." Carried unanimously.

Dr. Sedgwick, of London, proposed, and Dr. Coles, of Croydon, seconded,

"That the law which is numbered IX., be added to the laws of the Association." Carried unanimously.

Dr. Sedgwick, of London, proposed, and Dr. Royston, of London, seconded,

"That the law which is numbered XXVII., be added to the laws of the Association." Carried unanimously.

Some verbal alterations in the laws were then made, they were ordered to be re-numbered, and the amended laws as they are now printed were carried unanimously.

Dr. Ross proposed, and Dr. Royston seconded, the election of the officers for 1868. The list as now printed was carried unanimously, and Auditors were appointed for the next Anniversary Session.

The Session was continued on the 3rd.

Communications from Dr. Day, Dr. Procter, Dr. Edwards Crisp, Dr. Moffat, Dr. Nicholls, Dr. Wynn Williams, Dr. Drysdale, Dr. Lloyd Roberts, Dr. Shettle, Dr. Uvedale West, Dr. Mackinder, Dr. G. Buchanan, Dr. Smith, Dr. Junker, Dr. Richardson, F.R.S., Dr. Cordwent, Dr. Hughlings Jackson and Dr. Sedgwick were presented.

From press of matter some were read in abstract, and the four latter were withheld.

The President delivered an address on Research in Medicine.

Dr. Day, of Stafford, proposed, and Dr. Mackinder, of Gainsboro', seconded, a vote of thanks to the President for his powerful and eloquent address. Dr. Mackinder recalled the days in which the President and himself were fellow students at Glasgow, and said that the talent and originality of the youth had foretold a future in which the man would take rank with the ablest scientific Physicians of the age, an augury which had been amply justified.

A collection of Microscopes and microscopic preparations were exhibited by Mr. Collins, and Messrs. Murray and Heath; Surgical instruments by Messrs. Weiss, and Krohne and Seseman; Philosophical apparatus by Mr. Apps; and a complete series of his Miniature Dispensatories by Dr. Kirby

Dr. Edwards Crisp contributed an extensive and beautiful series of specimens, casts, and drawings of tubercle in man, and the lower animals, and a large collection of models of the eyes of animals. Mr. Ernest Edwards sent a large number of micro-photographs and cartes. Dr. Sedgwick exhibited a series of microscopic urinary deposits and sublimates, and, through the kindness of R. Barwell, Esq., F.R.C.S., an instrument for measuring the rotation of the spine, and photographs illustrating Mr. Barwell's views as to the cause of lateral curvature of the spine, and the results of paralysis of the *serratus magnus*. Dr. Mackinder, of Gainsborough, exhibited numerous pathological specimens.

Dr. Edwards Crisp, of London, proposed, and Dr. Hughlings Jackson, of London, seconded, a vote of thanks to Dr. Sedgwick, for the able and zealous discharge of his very onerous duties as Honorary Secretary. Dr. Crisp said that in his time he had belonged to many associations, but he had never met with so zealous and efficient a secretary as Dr. Sedgwick; his exertions were untiring, and he (Dr. Crisp) thought that the great success they had attained was mainly due to his efforts. He hoped, hereafter, although he knew that Dr. Sedgwick's exertions were entirely disinterested, that the Association would be able to afford him a more substantial mark of its gratitude.

Inspector General Dr. Leonard, of Norwood, proposed, and Dr. Day, of Stafford, seconded, a vote of thanks to the exhibitors for the great addition they had made to the attractions of the Session.

The Anniversary Dinner was held on the evening of the same day at Willis's Rooms. The President in the Chair. The Right Honourable Lord Stratheden and Campbell, Sir Edward Belcher, K.C.B., R.N., Sir W. Fergusson, Bart., Rev. Dr. Bell, Mr. Serjeant Robinson, J. Glaisher, Esq., F.R.S., the master of the Apothecaries' Society, the President of the Odontological Society, E. Harrison, Esq., Dr. Tilbury Fox, J. F. Clarke, Esq., Dr. Versman, and Dr. Silver, honoured the Association with their presence. Dr. Copland was prevented by sudden illness from attending the dinner.

Lord Campbell returned thanks for "The Houses of Parliament," and Sir E. Belcher for "The Army, Navy, and Volunteers."

The President proposed "Alma Mater," and Dr. Day proposed the health of the President.

Dr. Greenhalgh proposed "The Sister Universities," to which Dr. Tilbury Fox responded; Dr. Macintyre proposed "The Medical Corporations," to which Sir W. Fergusson and Mr. Cooper responded; Dr. Cholmeley proposed "The Learned Professions," to which Mr. Serjeant Robinson and the Rev. Dr. Bell responded; Dr. Ross proposed "Science, Literature, and Art," to which Mr. Glaisher, F.R.S., responded; and Dr. Rogers proposed "The Press, Medical and General," to which Mr J. F. Clarke responded.

I.

BUSINESS OF THE ASSOCIATION

AND

COMMUNICATIONS IN CONNECTION

THEREWITH.

REPORT OF THE COUNCIL.

At the First Anniversary Session of the St. Andrews Medical Graduates' Association, and on the presentation of their first Report your Council cannot refrain from very cordially congratulating the members on the establishment of such a Society, and on the hearty zeal and rapid progress which has marked its early growth; and they feel it their duty, at the outset, to put on record an account of the circumstances under which the Association has been formed, and the purposes which it is intended to serve.

When the present Government introduced the new Reform Bill for Scotland it was found to contain a provision for the representation in Parliament of the conjoined Universities of Edinburgh and St. Andrews, and the qualifications for the franchise were described as follows:—

"29. Every Person whose Name is for the Time being on the "Register of the General Council of any One of the Universities of "Scotland shall, if of full age and not subject to any legal Inca- "pacity, be entitled to vote in the Election of a Member to serve in "any future Parliament for such University in Terms of this Act.

"30. Besides the Persons constituting the General Council of "each of the said Universities, in virtue of the Sixth Section of the "Act Twenty-first and Twenty-second Victoria, Chapter Eighty- "three, every Person shall be entitled to be a Member of any such "General Council on whom the University has, after examination, "conferred the Degree of Doctor of Medicine, or Doctor of Science, "or Bachelor of Divinity, or Bachelor of Laws, or Bachelor of "Medicine and Master in Surgery, and who shall establish to the "Satisfaction of the Assistant Registrars, nominated as hereinafter "provided, that he has regularly attended during at least One "Session in such University, and in the Faculty to which the "Degree conferred on him belongs, Two or more Classes quali- "fying for such Degree; and the Name of every such Person

" shall be registered in the Book kept for the Purpose, on Payment " of the usual annual or other Fee : Provided always, that Doctors " of Medicine of the University of Saint Andrews on whom that " Degree has been conferred subsequent to the First Day of " January, One thousand eight hundred and sixty-three, shall be " entitled to be Members of the General Council, without having " attended any Classes in the said University."

In the Act for the regulation of the Universities of Scotland, alluded to in Clause 30, it is provided that the General Council of the University shall consist of " the Chancellor, of the Members of " the University Court from and after their First Election, of the " Professors, of all Masters of Arts of the University, of all Doctors " of Medicine of the University who shall have, as Matriculated " Students of the University, given regular Attendance on Classes " in any of the Faculties in the University during Four complete " Sessions, and also of all Persons who, within Three Years from " and after the passing of this Act, shall establish to the Satisfaction " of the Commissioners hereinafter appointed, that they have, as " Matriculated Students, given regular Attendance on the Course of " Study in the University for Four complete Sessions, or such " regular Attendance for Three complete Sessions in the University, " and regular Attendance for One such complete Session in any " other Scottish University, the Attendance for at least Two of such " Sessions having been on the Course of Study in the Faculty of " Arts."

With one or two exceptions, no medical man has graduated at St. Andrews in accordance with these terms, and thus the General Council of the University, numbering 368 persons, contains but two Graduates of Medicine of St. Andrews, and only one of them a Doctor ; although these constitute by far the larger number of the whole body of Graduates. At no time indeed, can any but a very small number of them be entitled to vote for a Member of Parliament for their University in accordance with the latter part of Clause 30 of the Reform Act; for, by the regulations regarding Medical Degrees of 1864, the University is prohibited from conferring the degree of Doctor of Medicine on a greater number of non-residents than ten in any one year.

It is very difficult to arrive at a correct account of the number of Medical Graduates of St. Andrews, but, after careful computation, your Council believe them to be about 1,300 in the United Kingdom, and of this number forty-six only will be admitted to the franchise by the proposed regulations. Such a state of things, so monstrously unjust and so full of danger to the well-being of the University, could not be allowed to continue without a vigorous attempt to bring about a better condition, and accordingly Dr. Richardson called together, at his house, a number of Graduates, for the purpose of united action against Clause 30 of the proposed Scotch Reform Act. At this meeting, held on the 29th of May,

1867, there were present, Drs. Richardson, Ballard, Wynn Williams, Ray, Rogers, Drysdale, Prosser James, Macdonald, Powell, Dudfield, Davies, Paul, Sedgwick, Griffith, Seaton, and Day Goss, and the following resolutions were unanimously agreed to.

"That an Association, to be called 'The St. Andrews Medical "Graduates' Association,' be constituted.

"That Dr. Richardson, 12, Hinde Street, London, W., be "appointed President; Dr. Paul, Camberwell House, Cam- "berwell, S., Honorary Treasurer; and Dr. Sedgwick, "2, Gloucester Terrace, Hyde Park, London, W., Honorary "Secretary.

"That an advertisement be inserted in the medical periodicals, "announcing briefly the constitution of the Association, and "that a circular letter to the same effect be sent to all "the Medical Graduates of the University in the United "Kingdom."

It was felt that the time had arrived when a permanent Association of the Medical Graduates of St. Andrews should be formed; for this was not the first time that their interests and status had been threatened. At one time an attempt had been made to exclude the University from representation in the Medical Council, and, at another, its degrees had not been recognised by the Poor Law Board. Each time a number of the Graduates living in London and the neighbourhood had to be hastily summoned; meetings were held, representations were made to the Government, and in both cases the opposition to the proposed injustice was successful. But these repeated attempts to lower the status of the University and its Medical Graduates showed the necessity of a more extensive and permanent organisation, and the response to the circular asking for support, proved that the recognition of this need was wide spread. In three weeks from the date of the meeting at Dr. Richardson's, when a General Meeting was held in the Freemasons' Tavern, for the passing of the laws of the Association and the election of the Officers and Council, 270 members had joined; and now the Association numbers in its ranks 402 Medical Graduates of the University of St. Andrews.

The first and primary object then of the Association is to obtain the franchise for all the Medical Graduates, and to secure the removal of that provision, which, dividing them into two classes—voters to the number of 46, and non-voters to the number of 1,250—marks the latter with a stigma which the general public will not be slow to recognise.

But participation in the election of a Member of Parliament is not the only object for which the Association should strive; that astounding absence of nearly all its Medical Graduates from the General Council of the University is a disfranchisement, the removal of which it is no less essential to accomplish. Your Council trusts that both these important and urgent needs, representation in Par-

liament and representation in the Council of the University, will, by the aid of this Association, be obtained for the Medical Graduates of St. Andrews.

By the periodical Sessions of the Association it is to be hoped that, according to their power, the Medical Graduates of St. Andrews may accomplish something for scientific medicine, and while doing their best to spread a deeper and more thorough knowledge of their own profession, they may be enabled to cultivate that breadth of mind which is to be derived from an acquaintance with the ever-growing truths of general science, and that refinement of thought and strengthening of intellect which comes of an occasional excursion into the wide fields of general literature. And so also another good may be effected: the members of a common University may be more closely drawn together by bonds of brotherly feeling and good-fellowship, and the example of the better and the wiser may have a broader and a deeper influence.

Your Council have now to report what has been done in regard to the franchise question.

It was thought right, first to address Mr. Ellice, the Member for the City of St. Andrews, and the President, the Honorary Secretary, and Professor Macdonald, had an interview with him. He requested them to put him in possession of all the facts of the case, especially as to the date of any alteration of the examinations; and, in consequence, the President wrote to the Registrar of the University for information, and received the following answer.

"University of St. Andrews,
"11th June, 1867.

"Dear Sir,

"I now beg to enclose excerpt Minute of the University, "containing the resolution, agreed to in 1833, regarding the con- "ferring of Medical Degrees. These regulations are, with but "slight alterations, the same as those adopted by the University in "1826—the main change being the appointment of extraneous "examiners. This system of examination was suggested by a "deputation from Edinburgh of several medical gentlemen of "the Medical School, viz., Mr. Liston, Dr. Macintosh, Dr. Robertson, "Mr. Lizars, and Dr. Gregory. These gentlemen were then "appointed by the University as examiners, and subsequently a "staff of examiners was appointed annually.

"There is *no truth* whatever that degrees were got by purchase. "During the war at the end of last century and beginning of the "present, degrees were granted to candidates obtaining appointments "in the army and navy, for instant duty, on certificate from three "or four *eminent* London physicians, &c., and these often were "rejected from the certificates being unsatisfactory."

"I am, dear Sir,
"Your obedient Servant,
"William Troup, *Cl. Univ.*"

The excerpt contains the regulations for the examinations, with which you are all familiar.

At a second interview with Mr. Ellice, this letter and its enclosure was shown him, and he then expressed a wish to know officially, whether before and after 1863 the examination was the same in extent. In reply to a letter of enquiry from the Honorary Secretary, the Registrar of the University wrote as follows:—"In reply to "your letter of the 13th instant (June), as to whether there is any "difference in the severity or extent of the medical examinations "previous to and after 1863, I beg to inform you there is no "difference in the examinations with regard to severity and strictness. "It may be stated, however, that since 1863, when the ordinance of "the Universities Commissioners came into operation, the examina- "tions have been only of candidates under the *first* section of the "regulations. These examinations necessarily embrace more of a "practical character, and are consequently not to the same *extent* as "would be the case of candidates presenting themselves under the "other sections of the regulations.

"I am unable to state anything regarding the feeling of the "University to which you refer, and it would be very difficult at "present—during the vacation—to procure a meeting of Senatus "for that purpose."

This was sent to Mr. Ellice, along with the following letter from the Honorary Secretary.

"2, Gloucester Terrace,
"June 20, 1867.

"Dear Sir,

"After I last saw you I wrote to the Registrar of St. "Andrews on the two points you named, the disposition of the "Senatus towards our claims, and the character of the examination "prior to 1863. I enclose you his reply, which I shall be obliged "by your returning at your convenience.

"The St. Andrews Medical Graduates' Association had a General "Meeting last night and about 100 Graduates were present. It was "intimated that in consequence of the many representations made "privately to Members by Graduates, the Lord Advocate had the "remodelling of Clause 30 under his consideration. May I ask "you if you think it advisable that the Association should ask for "an interview with the Minister so that he might know officially "the claims of a large body of the Graduates, or would you advise "a memorial from the Association?

"Pardon my troubling you with these questions, but we are "anxious not to lose a chance of a favourable consideration.

"Believe me, dear Sir,
"Faithfully yours,
"Leonard W. Sedgwick.

"E. Ellice, Esq., M.P."

To this there has been no reply.

Your Council then thought that under these circumstances they had better at once ask for an interview with a member of the Government, and as the Lord Advocate was not in Parliament they instructed the Honorary Secretary to ask Sir Graham Montgomery, Bart., M.P., Scotch Lord of the Treasury, for an audience. Sir Graham very kindly consented to see them, and fixed the 15th of July for that purpose at the Treasury. The deputation consisted of Drs. Richardson, Paul, Goss, Wynn Williams, Dudfield, Rogers, Ballard, Sedgwick, Prosser James, Ray, Greenhalgh, and Seaton.

Dr. Richardson stated the case, and, after explaining the claims of the Medical Graduates to a voice in the election of the Members for the University, read the following statement.

"The proposed new Reform Bill for Scotland, which provides for the representation in Parliament of the Universities of Edinburgh and St. Andrews conjointly, will practically exclude from the franchise all Graduates of St. Andrews who obtained their degrees before January 1, 1863. This exclusion is based mainly on the assumption that these Graduates were not examined for their degrees, but obtained them by purchase. As such an impression, which by the medical world is at once recognised as entirely and wantonly false, has been widely disseminated, and has even been conveyed to members of the Government, the Council of the St. Andrews Medical Graduates' Association instructed us, at their meeting of the 28th ult., to collate and publish the facts bearing on the examination and status of the Medical Graduates of St. Andrews. In obedience to this instruction, we have prepared the following statement, based on official information.

"The University of St. Andrews was one of the first to institute examinations for medical degrees, and has ever been foremost in adopting alterations to ensure the proficiency of its Graduates.

"It is utterly untrue that the St. Andrews degree can be obtained by purchase. The Registrar of the University writes:—'There is no truth whatever that the degrees were got by purchase. During the war at the end of last century and beginning of the present, degrees were granted to candidates obtaining appointments in the army and navy, for instant duty, on certificates from three or four *eminent* London physicians, &c., and then often were rejected from the certificates being unsatisfactory.'

"The examination lasts three days; it is party written and partly *vivâ voce*, and is in every respect similar to the examination for membership of the Royal College of Physicians of London.

"The University of St. Andrews was almost the first to institute examinations at the bedside.

"The examiners are unbiassed, a certain number of them being invariably distinguished Graduates of other Universities, and all of them being unknown to the candidates.

"The number of rejections is an evidence of the strictness of the examina-

tional test. In 1858, when the question of representation in the Medical Council was being considered, the official returns of the University showed that the rejections numbered one in four; a proportion higher than that of any other examining body, except the University of London; and this, although nearly all the rejected candidates were at the time legally-qualified practitioners.

"Another strong proof of the efficiency of the examination, and the clearest refutation of the objection, that the St. Andrews M.D. is not fitted for the franchise, is afforded by the statistics presented for the last three years to the Medical Council by the chiefs of the Army and Navy Medical Departments, which show the qualifications of the gentlemen who were examined by the Boards prior to employment in the services. As all the candidates must have a qualification before offering themselves for examination, the ratio of rejections must be considered conclusive proof of the value of each degree. We wish to make no disparaging remarks on other examining bodies, but we are bound in self-defence to draw attention to the following statement:—

32·52 per cent. of the Members of the Royal College of Surgeons,
18·18 per cent. of the Doctors of Medicine of Edinburgh, and only
8·33 per cent. of the Doctors of Medicine of St. Andrews,

failed to satisfy the authorities that they were fit to be trusted with the lives and limbs of our soldiers and sailors.

"If the severity of the examination tests in such a satisfactory manner the professional attainments of the Graduates in the earlier part of their career, the social and professional position to which many of them have afterwards attained, is an endorsement of the original approval. In London alone, thirty-nine Graduates of St. Andrews are attached to the Schools of Medicine, the Hospitals, and the larger Infirmaries: they hold important posts in St. Bartholomew's, Guy's, St. George's, Westminster, London, Charing Cross, Bethlehem, St. Luke's, Royal Free, Consumption, Queen Charlotte's, Samaritan, Great Northern, Cancer, Metropolitan Free, City of London for Diseases of the Chest, French, Soho Square for Women, and the West London Hospitals; at the Royal Infirmary for Diseases of the Chest, at the Ear Infirmary, and at the Farringdon, Finsbury, City, Marylebone, Pimlico, Islington, Western City, Peckham, and St. George's and St. James's Dispensaries.

"Doctors of Medicine of St. Andrews are also to be found in connection with Queen's College, Birmingham, and Newcastle-on-Tyne College; with the Schools of Medicine at Edinburgh, Dublin, Liverpool, Leeds, Manchester, Sheffield, Hull, and Sydenham College, Birmingham; with the Hospitals at Belfast, Waterford, Glasgow, Inverness, Leith, Dundee, Lincoln, Derby, Carlisle, York, Stafford, Nottingham, Birkenhead, Brighton, Bradford, Bath, Leamington, Winchester, Cheltenham, Halifax, Bristol, Rochester, South Staffordshire, Worthing, Chorlton, Portsmouth, Chatham, Coventry,

Worcester, Glamorgan, Wrexham, Stockport, Hertford, Middlesborough, Bournemouth, Weston-super-Mare, Devizes, Margate, Haverfordwest, Hartlepool, Sussex County, Sunderland, Limerick, and Queenstown; and with the County Asylums of Middlesex, York, Cambridge, Derby, Lincoln, Sussex, Surrey, Stafford, Lancashire, Cheshire, Worcester, City of London, Kent, Clonmel, Donegal, Inverness, Fife, Glasgow, and Edinburgh. They also number in their ranks two Deputy-Lieutenants, twelve Justices of the Peace, a Visitor in Lunacy, ten Coroners and Deputy Coroners, five Examiners in Medicine, and six Officers of Health.

"Three are Fellows of the Royal Society, eleven are Fellows of the Royal College of Physicians of London, and fifty-five are Fellows of the Royal College of Surgeons of England.

"B. W. RICHARDSON, M.D., F.R.S., *President.*
"LEONARD W. SEDGWICK, M.D., *Hon. Sec.*"

After hearing other members of the deputation in support of the claim, Sir Graham Montgomery said that the Lord Advocate was unable to be present, owing to an appeal case in which he was engaged, but promised to lay the matter fully and fairly before his Lordship. He thanked the deputation for the information they had given him, and for the important statement they had placed in his hands, and expressed himself to be "favourably impressed" with the claim. He added that this question was a matter for consideration in committee, not on the second reading, and that the bill would not go beyond that stage during the present session; but if the deputation wished to see Mr. Disraeli next year he would be very glad to obtain for the Council an interview wtth the right honourable gentleman.

This then is the position at the present moment of the franchise question.

Your Council thought that the University authorities would wish to support the claims of its Medical Graduates, and at its first meeting requested the Honorary Secretary to write fully on the matter to the Senior Principal. The following is the letter written.

"2, Gloucester Terrace, Hyde Park, London, W.
"July 10th, 1867.

"Sir,

"At the first meeting of the Council of the St. Andrews Medical "Graduates' Association, the following resolution was unanimously "adopted. 'That the Secretary be requested to address a letter "'to the Senatus Academicus of the University of St. Andrews, "'stating that a St. Andrews Medical Graduates' Association has "'been formed, enclosing its rules and the number of its mem-"'bers, and asking for the counsel and assistance of the Senatus "'in the removal of the disfranchising clause of the new Scotch "'Reform Bill, and in the furtherance of the other objects of the "'Association.'

"In accordance with this request, I ask permission to address "the Senatus Academicus through you.

"On several previous occasions it has been found necessary to call "together some of the London Graduates of St. Andrews to consti- "tute a committee for the defence of the rights of the University and "its medical members; notably when the University was refused a "representative in the Medical Council. But now, when by the "same Act which grants a representative in Parliament to the "Universities of Edinburgh and St. Andrews, the Doctors of "Medicine of St. Andrews are almost in a body excluded from a "voice in his election, it was thought that the time had arrived to "originate an Association on a wider basis and of a more perma- "nent character.

"Impressed with this idea, a number of the Medical Graduates "met, and resolved on the constitution of such a Society. At the "first General Meeting held on the 19th ult., no less than 265 "Graduates had already joined, and now the Association numbers "upwards of 300 members.

"The Association has for its objects the advancement of the "science and art of medicine, and of general science and literature, "the maintenance of the interests of the Medical Graduates "of the University, and the cultivation of social intercourse and "good-fellowship.

"In the furtherance of these laudable aims, the members of the "St. Andrews Medical Graduates' Association earnestly desire the "sympathy and aid of the Senatus Academicus of the ancient "University to which it is their pride and their privilege to belong. "They ask this aid all the more confidently because they know "that the Senatus is ever wishful to be foremost in defence of the "rights of the Graduates of the University, and because they feel "sure that the objects of the Association will commend themselves "to the approval of all well-wishers of St. Andrews.

"In regard to the franchise it is felt that all Doctors of Medicine "by examination should be placed on equal terms; all have equally "complied with the regulations of the University, and all should "equally partake of any privileges which attach to the degree they "have obtained. This position seems so just that there would appear "to be no appeal from it. But it has been contended by persons high "in authority, first, that the degree is obtainable by purchase, and "secondly, that the examination for M.D. of St. Andrews is so lax "that a large number of incompetent persons have been admitted. "Of course it is not necessary to enter into any serious refutation of "this calumny with the Senatus Academicus, but I would venture "to draw their attention to the enclosed statement,* which has been "sent to all the Graduates in accordance with the recommendation "of the Council; and I would also in their name respectfully request

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* See page 12.

"the Senatus, in their official capacity as a governing body of the "University, publicly to protest against these injurious assertions.

"The Members of the St. Andrews Medical Graduates' Association, "as you will notice by the rules, a copy of which I have the honour "to enclose, purpose having a General Session every year. The "first will be held in London early in December next. They propose "to devote the first day to the discussion of matters intimately af- "fecting the University and its Medical Graduates; the second to "the consideration of medical and scientific subjects. In the evening "they dine together. They thus seek to bind in closer bonds the "members of a common University, that so they may in concert and "combination perform more worthily the duties which pertain to "them as a division of the great army of Medicine, and do more "than they have ever yet been able to do in support of the credit "and honour of their venerable Alma Mater.

"With these aims and in this spirit, I have the honour to address "the learned body of which you are President, and to ask them to "encourage by their generous sympathy and their active support, "the St. Andrews Medical Graduates' Association.

"I have the honour to be, Sir,

"Your faithful Servant,

"LEONARD W. SEDGWICK, M.D.,

"*Hon. Sec, St. And. Med. Grad. Ass.*

"To the Very Revd.

"PRINCIPAL TULLOCH, D.D,

"*President of the Senatus Academicus*

"*of the University of St. Andrews.*

As the vacation of the University only ends in November, the Senatus have not yet had an opportunity of considering the matter and replying to the request.

Your Council have found great difficulty in arriving at a knowledge of the objections to the admission of the Medical Graduates to the franchise of their University, but two have been prominently urged. The first, and this has been brought to the notice of a Member of the Government, is that the St. Andrews degree conveys no proof of scientific knowledge, inasmuch as the excluded Doctors have not been examined, but have bought their Degrees. To refute this calumny, your Council requested the President and Honorary Secretary to draw up a statement of facts in connection with the examination at the University and the professional status of its Graduates. This statement, which is quoted above, your Council conceive to be a sufficient proof of the utter baselessness of this frequently repeated objection.

Another reason given for the exclusion of the Medical Graduates of St. Andrews from the franchise, is that most of them are Englishmen, and should not have a vote for a Scottish University. Independently of the anachronism of the objection, which belongs

to times long gone by, when the interests of the two sections of the nation were not the same, and when there was a real, and not as now, a mere nominal separation between England and Scotland, it is also unfair, for the nationality of the Graduate is in no other University a cause of disfranchisement. Scotchmen and Irishmen vote for the Members of English Universities, Englishmen and Scotchmen for those of the Dublin University, and Edinburgh Graduates were not disqualified by the proposed bill if they were Englishmen. Retroactive legislation is admittedly unjust, and the Medical Graduates of St. Andrews having complied with all the regulations of their University should not by such means be arbitrarily deprived of a right which in all other Universities appertains to the degree of M.D.

Your Council have thought it right to offer for your consideration some modification of the laws of the Association, and hope to have your permission to publish the proceedings of the Sessions.

The report of the Treasurer shows the finances to be in a satisfactory condition.

Your Council regret to announce that since the formation of the Association one member, Dr. Norton, of Martham, has died.

Your Council in conclusion would earnestly press upon every Doctor of Medicine of St. Andrews the necessity of united and vigorous action, for so only can they obtain their rights as freemen and as graduates, and so only can their venerable University, hoary with the age of centuries, and rich with the memories of great men, become a living moving power in the future of Medicine.

B. W. Richardson, M.D., F.R.S., *President.*

Leonard W. Sedgwick, M.D., *Hon. Sec.*

December 2, 1867.

REPORT OF THE TREASURER.

No detailed statement of the finances of the Association, as required by the laws, can at this, the first Anniversary Session, be presented to the members. Careful estimates have been made, and it is believed that the receipts will be sufficient to meet the whole expenses of the year, and leave a small surplus.

J. H. PAUL, M.D.,

Hon. Treasurer.

December 2nd, 1867.

THE UNIVERSITY OF ST. ANDREWS, AND ITS MEDICAL GRADUATES.

By Leonard W. Sedgwick, M.D., Hon. Sec. of the Association.

It has been my duty as Honorary Secretary of the St. Andrews Medical Graduates' Association, to make myself acquainted with the constitution and government of the University, and its relationship to the Doctors of Medicine, and as I believe that much that I have learnt, will be new and interesting to the majority of the members of this Society, I have felt myself justified in asking your attention for a short time to this important matter, and I trust that I shall be able to shew, that here, in the regulations of the University itself, is the root of the evil against which we are met to contend; here, the cause of our hitherto divided and powerless condition, like to the great Arab Chief, with every one's hand against us, but unlike him, with no arm of our own for defence and protection.

The University of St. Andrews from its locality has always been, and must continue to be, in so far as medicine is concerned, an examining, not a directly educating body. Without a large Hospital, it could not be a school of Physic, and so its Doctors of Medicine, though in every sense of the word Graduates, have, with very few exceptions, never been students of their University.

The University of London has a similar function, and while regulating the mode in which its Graduates shall obtain their knowledge, it offers no teaching itself, and it allows the student a wide latitude in his selection of a place of study.

The other Universities are directly educational institutions as well as examining bodies.

On the present occasion I wish, without going very fully into the relative value of these two different ideas, and their applicability to the profession of medicine, to present some views in regard to them which seem to lie on the very surface of the matter, and also to direct your attention to the status of the Medical Graduates of St. Andrews in their University, to contrast it with the position that the Doctors of Medicine of other Universities occupy, and to offer for your consideration some suggestions for the removal of an injustice which most nearly affects us all.

I will in the first place relate very briefly the constitution of the chief Universities in so far as the Medical Graduates are concerned, And for this purpose it is not necessary that I should trouble you

with details of the different powers, the Senate, the Council, the University Court, or whatever they may be called, but I need merely address myself to a short description of the general body of the University, and shew the position the Medical Graduates have in it. This body is called variously in different Universities, but it has mainly the same functions, and is the primary source of power.

In Oxford it is called Convocation. It consists of all Masters of Arts, and all Doctors of the three superior faculties, who have their names upon the books of some College or Hall. All matters of legislation come before this body. Professorships and livings, which are elective, are filled by its votes, and so also are Members of Parliament chosen.

A similar body in the University of Cambridge is called the Senate. All Masters of Arts, or Laws, or Doctors in the faculties of Divinity, Law, and Physic, having their names upon the University Register have votes in this Assembly. This body exercises legislative functions, and elects the Members of Parliament.

The Convocation of the University of London is composed of all Doctors of Laws, Doctors of Medicine, and Masters of Arts, of all Bachelors of Law, and Bachelors of Medicine of two years standing, of all Bachelors of Arts of three years standing, of all Doctors of Science, and of all Bachelors of Science of three years standing, who have kept their names on the books, and have paid their fees. Convocation has the power of discussing any matter whatsoever relating to the University and declaring its opinion thereon, and it elects the representative in Parliament of the University.

Similarly in Dublin University the Senate or Congregation is composed of all Doctors of the different Faculties, and of all Masters of Arts, and it elects the members of Parliament.

If the proposed Scotch Reform Bill should pass in its present shape, Edinburgh would be in a similar condition. At present the General Council, which is a similar body to the Senate or Convocation of the other Universities, has on it only those Doctors of Medicine who have attended four Sessions in the University. But the new bill provided that, All Doctors of Medicine, Doctors of Science, Bachelors of Divinity, Bachelors of Laws, and Bachelors of Medicine, and Masters in Surgery, who have attended during one Session in the University should be entitled to be placed on the General Council, and as the University of Edinburgh admits to its examinations those who have attended only one Session in it, the Medical Graduates would, if this bill became law, occupy the very same position they do in all other Universities. Now as it is not at all likely that whatever modifications may be made in the bill, this part of the clause will be removed, we may for our present purpose look on the Medical Graduates of Edinburgh as having the same rights as those of the other Universities. I would note in passing the

accuracy with which the minimum of residency required by the bill, tallies with that demanded by the University for its Medical Degrees.

In all these noble institutions then, every Doctor of Medicine has a voice in the government of his University, and a vote for its representative in Parliament. Nationality is nothing; if a man holds a degree of M.A. or one above it, keeps his name on the books by the payment of the proper fees, and obeys the laws of his country, he is not deprived of the rights pertaining to his degree on any pretence whatever.

Now let us turn to St. Andrews, to the University, the breath of whose life for years has been its Medical Graduates, and let us see how it is governed. First there is the General Council, which consists of the Chancellor and the other members of the University Court, from and after their first election, the Professors, all Masters of Arts of the University, Doctors of Medicine of the University, who have as Matriculated Students of the University, given regular attendance on Classes in any of the Faculties in the University during four complete Sessions, and of all who, within three years after the passing of the Scottish Universities Act (2nd Aug. 1858), established that as Matriculated Students, they had attended the University for four complete Sessions, or for three complete Sessions in the University, and a fourth at another Scottish University, the attendance for at least two of such Sessions having been in the Faculty of Arts. All members of the General Council must be twenty-one years of age, and must have their names registered in a book kept for that purpose by the University. Each member on being first registered must pay a fee of five shillings, and also afterwards two shillings and sixpence yearly on the fifteenth of October for the year ensuing, or a composition fee of one pound. The General Council meets twice a year; it elects the Chancellor and one of the Assessors in the University Court, and it is empowered to "Take into consideration all questions affecting the well-being and prosperity of the University, and to make representations from time to time on such questions to the University Court, who shall consider the same, and return to the Council their deliverance thereon."

Secondly, there is the Senatus Academicus, composed of the two Principals and the Professors of both the Colleges. It has the ordinary superintendence and regulation of the teaching and discipline of the University, and the administration of its property and revenues, subject to the control and review of the University Court.

And lastly there is the University Court itself. It consists of the Rector, the Senior Principal, an Assessor nominated by the Chancellor, an Assessor nominated by the Rector, an Assessor elected by the General Council, and an Assessor elected by the Senatus Academicus. The University Court is invested with the following powers.

1st To review all decisions of the Senatus Academicus, and to be a Court of Appeal from the Senatus in every case, except as otherwise provided in the Universities Act.

2nd To effect improvements in the internal arrangements of the University, after due communication with the Senatus Academicus, and with the sanction of the Chancellor; provided that all such proposed improvements shall be submitted to the University Council for their consideration.

3rd To require due attention on the part of the Professors to regulations as to the mode of teaching and other duties imposed on the Professors.

4th To fix and regulate from time to time the fees in the several Classes.

5th Upon sufficient cause shown, and after due investigation, to censure a Principal or Professor, or to suspend him from his office and from the emoluments thereof, in whole or in part, for any period not exceeding one year, or to require him to retire from his office on a retiring allowance, or to deprive him of his office; and, during the suspension of any Professor, to make due provision for the teaching of his class: provided always that no such sentence of censure, suspension, or deprivation, or requisition on a Professor to retire from his office, shall have any effect until it has been approved by her Majesty in Council.

6th To enquire into, and control the administration by the Senatus Academicus or Principal and Professors of any College of the revenue, expenditure, and all the pecuniary concerns of the University and of any College therein, including funds mortified for bursaries and other purposes.

And where, we may well ask is the representation of the Faculty of Medicine here?

The University Court consists of six members, of whom one is a Doctor of Medicine, but not of St. Andrews; he is an Edinburgh Graduate.

The Senatus Academicus consists of fourteen members, three are M.D.'s, but again all belong to Edinburgh.

The General Council consists of 368 members, of whom eleven are M.D.'s, and here is the representation of the Medical Graduates of St. Andrews, for there is one Doctor and one Bachelor of Medicine of the University on it.

But perhaps the Graduates in other Faculties are so numerous, that after all, this may be a fair proportion. Listen to what the Registrar of the University says in a Return presented to Parliament last Session on the motion of Mr. M'Laren. "There are at present 368 members of the General Council; a further number of sixty-nine are entitled to become so on payment of their fees, thus making 437." This is the number of all the non-medical Graduates of the University. The Medical Graduates are some 1,300, and thus a minority numbering just one quarter of the whole is master of

the position. But, it may be urged, the proposed Reform Bill would modify this, inasmuch as all M.D.'s who have passed since 1863 would be added to the General Council. It is true, they would, and they amount to forty-six; but what are forty-six out of 1,300, and as yet even these are not. So at the present time remains the government of the University of St. Andrews, and so are treated its Medical Graduates; not as in other Universities equal in rights and in privileges to their fellow Graduates, but an outcast class, excluded, unrecognised. Strange contrast this little world to the outer and the greater. In the one every ratepaying householder, gentle and simple, rich and poor, learned and ignorant, alike having his vote for the man who is to speak for him in the Great Council of the nation; in the other, a large and intelligent majority, men highly educated and yet practical, whose daily duties require them ever to be reducing thought into action, the very persons if there be such entitled to self-government, tongue-tied and powerless, without a vote or a voice.

Surely such a state of things should not be allowed to continue; it seems a marvel that it could ever have arisen. But monstrous as is the injustice, it is certain to last unless we bestir ourselves actively; not in a momentary burst of enthusiasm, but with a deep conviction of the need of steady earnest work; for the power of inertia is great.

If I have made this account of the Government of the University clear to you, I have I think in the very narrative pointed out the root of the present evils. It is our own utter want of connection with it. We pass our examination, pay our fees and are capped, and then we go; Alma mater knows us no more. With no one to speak for us, no one to care for us, what wonder that our interests are never considered, our rights and privileges never asserted. Scattered over the whole kingdom, with no bond of connection, we were powerless until this Association was formed, but now I trust another era has begun for us, and united we shall make ourselves heard and felt. Reform, then is needed, and it is simple, as it is necessary; just, as it is desirable. Simple, because we require only to be put upon the General Council of our University; just, because what we ask is only equal rights with our fellow-Graduates. If clause XXX of the Scotch Reform Bill were modified as follows, we should obtain all that we could desire in this matter.

"Besides the persons constituting the General Council of each of the said Universities in virtue of the Sixth Section of the Act Twenty-first and Twenty-second Victoria, Chapter Eighty-three, every person shall be entitled to be a member of any such General Council on whom the University has, after examination, conferred the degree of Doctor of Medicine, or Doctor of Science, or Bachelor of Divinity, or Bachelor of Laws, or Bachelor of Medicine and Master in Surgery, and the name of every such person shall be registered in the book kept for that purpose, on payment of the usual annual or other fee."

This is clause XXX as it now stands, omitting simply the paragraph which restricts the privilege to those Graduates who have attended in St. Andrews, in the faculty to which the degree conferred on them belongs, two or more classes qualifying for such degree. The disfranchising paragraph in short. We should thus be placed on the General Council of the University, and as the previous clause confers the franchise on all members of the General Council, we should obtain at the same time, a voice in the Government of our University, and a vote for its representative in Parliament. The University of St. Andrews has never required, and could not require of the Candidates for Medical degrees, residence within its walls as a *sine qua non*, but neither does the University of London, and Edinburgh asks for only one year as a necessity. Is it not then unjust in the highest degree that we, who have complied with all the regulations of the University, and have obtained its degree of M.D., should be deprived of the dearly-prized right of a free tongue and an equal voice, which the Medical Graduates of the other Universities possess ?

I need not say more on this point, but there is another matter which, in the interest of the University itself requires revision, and and that is the following regulation : "The degree of Doctor of Medicine may be conferred by the University of St. Andrews on any registered Medical Practitioner above the age of forty years, whose professional position and experience are such as, in the estimation of the University, to entitle him to that degree, and who shall, on examination, satisfy the Medical Examiners of the sufficiency of his professional knowledge : provided always, that degrees shall not be conferred under this section to a greater number than ten in any one year." If it be right to grant ten degrees under these circumstances, what possible reason can there be for refusing to grant fifteen or any other number, if so many candidates offer themselves, and are able to pass the examination. No principle is concerned in this charmed number ten, neither is there any principle concerned in the limitation by numbers at all. Its action, whilst unjust to all candidates beyond the ten who may be allowed to graduate, must be injurious to the University by cutting off its supplies. The framers of the regulation doubtless intended by its means to render it impossible for any unfit person to obtain the degree ; at least there is no other conceivable purpose to be effected by it, consistently with an honest wish for the welfare of the University. But speaking with all deference and respect to the distinguished Commissioners, under whose instructions it was framed, I cannot but call it a clumsy and fatal device. For by it the earliest applicants and not the most worthy candidates, will be admitted to the degree, and at no distant period the University must die out by a process of starvation. A far better plan now exists for securing a satisfactory examination, and that is the power of visitation possessed by the Medical Council. Year by year this will become more of a reality

and less of a form, and even now it has borne good fruit in a very much improved examination, at more than one of the Medical Corporations. It would enhance the worth of the degree, not by limiting the number of candidates, but by exacting a high standard of acquirement.

There is a still further consideration to be entertained; assuming that the alteration of the regulations which I have indicated is a desirable one, is it also practicable. There is a general impression abroad that to effect such a change an Act of Parliament is necessary, and this would be beyond our power. But the matter does not stand thus. On reference to the Scottish Universities Act, which I have quoted so frequently, it will be found that there are no regulations for the granting of degrees there, but that by it, twelve noblemen and gentlemen, with our present Chancellor, His Grace the Duke of Argyll, at their head, were appointed Commissioners, and empowered among other things, to "make rules for the management and ordering of the saidUniversities, the manner and conditions in, and under which Students shall be admitted thereto, the course of study and manner of teaching therein, the amount and exaction of fees, the manner of examination with the qualifications, appointment, and number of examiners, and the amount and manner of their remuneration, the granting of degrees, whether in Arts, Divinity, Law, or Medicine, and to provide that, in so far as shall be practicable, and in the opinion of the Commissioners conducive to the well-being of the Universities, and to the advancement of learning, the course of study, the manner of examination, and the conditions under which degrees are to be conferred, shall be uniform in all the Universities of Scotland."

But these Commissioners who, by this authority, have drawn up the regulations for the granting of degrees in Medicine which are now in operation, were not a permanent body. The Act declares that the "Powers hereby conferred on the Commissioners shall be in force until the first day of January, one thousand eight hundred and sixty-two, and it shall be lawful to Her Majesty, by and with the advice of Her Privy Council, to continue the same until the first day of January, one thousand eight hundred and sixty-three, and no longer." So they are defunct now, and the University Court is the ruling power in their stead, for what says the Act?—Clause XIX. runs thus:—"During the subsistence and exercise of the powers of the Commissioners, the powers hereinbefore conferred on the University Courts shall be exercised in subordination to, and so as not to conflict with the powers of the Commissioners. But any of the Rules, Statutes, and Ordinances to be framed and passed by the Commissioners, as hereinbefore provided, may, at any time after the expiration of the powers herein conferred on the Commissioners, be altered or revoked by the University Court of the University to which the same are applicable, but only with the consent, expressed

in writing, of the Chancellor thereof, and with the approval of Her Majesty in Council."

The UNIVERSITY COURT then is the body to which we must appeal, to it we must carry our grievances, and by it, with the consent of the Chancellor of the University and of the Queen in Council, they will surely be remedied. For it cannot be doubted that if we can prove the wrong, it will not long be allowed to continue.

But we must not ignore the existence of objections to the possession by the University of any power at all to grant the degree of M.D. to a non-resident.

Independently of the long possession of this power by the University, and which, in common justice, should not be withdrawn except for good and sound reasons, it is not difficult I think to shew that such a privilege is most desirable and advantageous to the University, the profession, and the public.

The University of St. Andrews has no medical school, no large hospital to attract students, and if it is to grant Medical degrees at all, it must be to non-residents; for if medical knowledge is required, a student must go elsewhere to obtain it.

Again, it is most desirable for the Medical Profession that, protected by the one safeguard, a stringent examination, any of its members should at any time, if so disposed and competent, be able to obtain the Doctorate. It is quite unnecessary that I should instance examples of the need of such an University as that of St. Andrews, but I cannot refrain from directing your attention to an advertisement in last week's Medical Journals for an Assistant Physician to the Consumption Hospital at Brompton.—"Candidates must be Members or Fellows of the College of Physicians of London, *and* Doctors, or Bachelors of Medicine of a British University." Suppose a man, after some years of practice, led by inclination or accident, to devote himself chiefly to the study of chest diseases; suppose him, (no unnatural idea), wishful to be connected with this special hospital, he can without any residence at any special place, become a candidate for the Diploma of the College of Physicians, but unless he is more than forty years of age, and unless fewer than ten men have applied to be examined before him, he cannot during that year obtain the M.D. degree. When the next year comes the chance may have gone for the hospital appointment. Can there be a more cogent reason for leaving the University doors open to such an one, provided he can pass the required examination? You may say that when a man has been some years in practice he will not be at the trouble to "get up" Minute Anatomy, Chemistry, Physiology, or the refinements of Medicine and Surgery, and would not be able to pass a sufficiently satisfactory examination if he tried. I do not for a mement believe it. Very many here present are practical refutations of this objection. But even if it were so, the existence of a stringent examination would itself prevent any evil

accruing in this way, for if no one of such men could pass, no one would pass. Prevent a man from offering himself because you think he could not satisfy the examiners, and you presumptuously bend facts to your theory. Provide a stringent examination, and the facts take care of themselves, there is no need of a theory.

This does not at all touch the question of how best to educate the future medical practitioner. No doubt several years spent in study at the outset of his career, in accordance with the regulations of such a University as that of London, is of all ways of obtaining medical knowledge, for most people the best. But the best for most is not the best for all, and the best cannot always be done. If from a scanty purse or other circumstances, a man has to content himself with the minimum of time for study, and forego the honour of a Doctor's degree, because he can live only by the sweat of his brow, is that a reason for shutting him out from it for ever. The whole spirit of modern British legislation and thought is against such a monopoly, such an exclusion. Granted that it is difficult when harassed by the cares and worries of actual practice, with others dearer than one's self to provide for, to keep pace with the scientific progress of the day, and to satisfy the requirements of a University examination; all the more honour is due to the man who successfully accomplishes it, all the more reason to put no unnecessary difficulty in his path.

But this provision for which I am contending is eminently advantageous to the public. It is of all things for it most important that every stimulus should be given to the acquirement by the medical practitioner of knowledge beyond the more immediate and necessary contingencies of the day; most important that when circumstances demand it, a rising fame, a more responsible position, a wider sphere of usefulness, the coveted title should be within the reach of every worthy aspirant. Honours are not like old china, valuable only in proportion to their rarity.

There is one other matter to which I would briefly draw your attention, and that is the advantage which would accrue to the University and the profession, if the authorities of St. Andrews would recognise as one of their special missions, the education of young men in the Natural Sciences for a year or two after they have left school. Health would be promoted, time would be saved, and medical knowledge would be more easily and solidly obtained, did Students by this means, go to the Medical Schools with a good knowledge of Physics, Chemistry, Botany, and Natural History.

But I must not pursue this part of the subject further. I fear I must have wearied you, for I am as conscious of the weakness of the advocate as of the strength of the cause.

At the conclusion of Dr. Sedgwick's paper,

Dr. Coles observed that, judging from the report of Council, and the excellent paper just read, it was of the first importance that the Medical Graduates should, if possible, be placed on the Council of their University, as they would thereby obtain not only the franchise but a voice in the management of the University, and enquired if the authorities at St. Andrews had the power of themselves to place them on the Council, irrespective of the Scotch Reform Bill, provided they were disposed so to act.

Dr. Davey (Northwoods): It would be strange indeed if the reading of the excellent paper by Dr. Sedgwick did not provoke a warm and an interesting discussion. I rise for the purpose of expressing my decided approval of the principle of *representation*, as shadowed forth by the author. To ignore it, would be to place ourselves in direct opposition to *the* question of the day, and to what has now become a *fashion* (if I may so speak) with each and every party in the great social fabric. The inconsistencies, so well explained and detailed, in the present constitution of St. Andrews University can be but temporary, provided we Graduates are in earnest, and really mean to press the consideration of reform and progress in the right quarter. Doubtless the undue preference given to those gentlemen who have obtained their M.D. degrees since 1863, must not be. The older Graduates will have enough self-respect to ask for and obtain a fair and equal representation with their fellows. In a body of men, each one being equal to any other, on what grounds can *monopoly*, in any sense, be else than condemned. This is a matter, Mr. President, which is hardly more interesting to us as Graduates of the old University we represent this day than—it is one which presses heavily on us—as Members or Licentiates of the Medical Corporations in Pall Mall or Lincoln's Inn Fields. The Colleges of Physicians and Surgeons are to this time presided over by little else than a self-elected few. Where are the many among other Members and Licentiates? Left out in the cold, unheeded, and uncared for. This should not be; nor will such a state of things long continue. Look farther—at the Medical Council. What does this body represent, for the most part? Simply itself, to the exclusion of the registered practitioner. With these things we, I think, must one day deal; and in the same spirit, and with no less force and earnestness, than is apparent among us on this present and especial occasion.

Dr. Crisp thought that Dr. Sedgwick, whose paper he had listened to with great pleasure, had gone to the root of the mischief, and until the members of the various corporate bodies had a voice in the management of their own affairs, there would be no efficient reform in the profession: he belonged to three of these bodies, and he had written to all to know how the money was expended, but he could get no satisfactory answer. Look again at the constitution of the Medical Council, a body elected by a few of

the exclusives of the profession. The general practitioners who constituted the vast bulk, forty-nine fiftieths of the medical practitioners of this kingdom, were entirely unrepresented, although for more than thirty years they had been praying for a representative form of government; there were signs abroad, however, that he thought indicated a better state of things for the future.

Dr. Shorthouse spoke strongly against the injustice that was proposed to be inflicted on a large and influential body of men, and drew attention to the inconsistency of the authors of the bill in conferring the franchise on the Doctors of Medicine of the University of London, who are all non-residents, and refusing it to those of St. Andrews on the score of non-residency. He very heartily concurred in the proposals advocated by the Honorary Secretary.

Dr. Sedgwick, in reply, thanked the members for their kind reception of his paper, and in answer to Dr. Coles, said that the University authorities had no power to put any of their Graduates on the General Council, but that the clause which, in the last Scotch Reform Bill, was so objectionable, would, if amended as he proposed, give to all the Medical Graduates a seat in the General Council, as a necessary preliminary to a vote for the member of Parliament.

II.

COMMUNICATIONS

ON

MEDICAL AND SCIENTIFIC SUBJECTS.

THE INAUGURAL ORATION

ON RESEARCH IN MEDICINE.

By Benjamin W. Richardson, M.A., M.D., F.R.S.,
Fellow of the Royal College of Physicians.
President of the Association.

The honoured position to which you have elected me as your President is not less to be appreciated than to be feared. The office, while it bears such recognition as all earnest men must love, carries with it responsibilities which the least timorous must feel. We are making in this associational effort an experiment of power in the face of a critical world. The University we represent, encrusted with centuries of learning, can afford to have its name linked with no poor art; what has been, has been of the best; what is to be, must, in some sense at least, partake of what has been.

In that part of the University which we may call the medical part this care is more than ever demanded. Medicine, as represented in the University of St. Andrews, is under disadvantage. There no great school of Physic exists in which the skill of the master may find scope for labour. Those who take Esculapian honours from our University must gain their knowledge elsewhere, and merely register it in the College calendar. Hence it has been common for the more favoured seats of medical lore to look upon us with some distrust; distrust which it is our duty to remove; to remove not by boasting, not by contention, not by complaint, and certainly not by despair; but by a manly and truthful exposition of our power by the side of power, of our industry by the side of industry, and by an earnest determination to prove that, after all, we are worthy of our vocation; in a word, by fair and noble and ennobling competition with our peers. Of us especially it must never be said

> "Our sails flap idly when our busy prow
> "Should grate the golden isles.

Recasting many times in my mind what on my part would most become this occasion, I have pushed aside many topics to light at last on one which comes to me most naturally, and, by habit of thought, most powerfully, I mean *Research in Medicine.* How shall we proceed best in improving the glorious work which by the divine ordination lies before us: how shall we save pain, how shall we remove disease, how shall we prolong life? These are the questions which should ever be present in the broad expanses of our thought, as in the details of our practice.

If, in the ideas which I shall offer, I should indicate courses of labour and preparations contrary to much accepted view, let me as a preliminary say, that whereas unquestionably I speak to influence, to influence many indeed besides those who do me the honour here to listen, I would not, if I could, speak by art to dogmatize, nor by stratagem to convince, nor by flattery to please, nor by poetry to fascinate: but I would speak, if it were possible, with firm honesty of purpose that which seems to me true, though it be heresy and even error itself.

ON THE FRAME AND CONSTITUTION OF MIND FOR MEDICAL RESEARCH.

The very first necessity in medical research is a proper frame and constitution of mind. This condition, partly natural, partly acquired on a natural talent, is such that it ruthlessly and inevitably reduces to a small minority all who truly live, and who, when they are dead, command. So great is this limitation that the twenty three centuries from the Father of medicine have not brought twenty masters who, at this moment, are powerful to command. Hippocrates still holds out the natural history of disease; Paulus remains the foundation stone of surgery; Paracelsus keeps the crucible; Vesalius as yet is the anatomist; Harvey still has his hand on the engine ofthe circulation; Willis is opening the skull case and unrolling the brain; Mayow continues to teach that there is a furnace in the animal body burning by the air; Black and Priestly tell the nature of the combustion; Haller adds physics to physic, and Boerhaave scientific chemistry. Pinel puts the psychical upon the physical; John Hunter links the physiology of animals inferior to that of the animal superior; Jenner stands out alone the revealer of a wholesale remedy; Humphrey Davy, escaping from his nitrous oxide box, and exclaiming to Dr. Kinglake, "Nothing exists but thoughts, the universe is composed of impressions, ideas, pleasures, and pains," —leads the beneficent advance of those who have abolished the horror of the surgeon's knife; and Laennec, pronouncing a diagnostic on such safe physical basis as to leave no improvement on his principles, heads the last of the—

"Tongues of our dead not lost,
"But speaking from death's frost
"Like fiery tongues at Pentecost.

To reach to the height of these men, not one of whom was indebted to accident for his success, the frame and constitution of mind is so rare that few are likely to near it. It is not difficult, nay it is very easy, for a man to become a great performer in the art of physic, especially in the chirurgical part, and, thus excelling, to become justly famous; it is easy for a man to become erudite; it is comparatively easy for a man of ingenious mind to become a

great theorist, and, by his speculations, to live even briefly after his death; Cullen, Darwin, and Brown are noted examples in this line. Lastly by a spick and span method of ignoring fixed truths and inventing wild dogmas, it is the easiest of all things to gain a spurious fame, and even to live, as Hahneman has long lived, on the uplifted ignorance of the illiterate in science.

But the medical science which advances truly comes from labour developed far differently from aught or any of these last named qualities. It is a labour *sui generis* —The mind that yields it must have one primary attribute—*impassion*. If a single earthly object has to be served by the labour, and that be its design, assuredly the labour is damned forthwith. The mind must be fervid to the extreme of tension, and rather break than yield, or no clear and certain sound shall be told. The mind to external friction must be smooth, relentless, impervious, otherwise impressions of fools may indent it and destroy its true nature. The mind, lastly, must be a mind within a mind, renewing itself, and correcting itself by the renewal, without any care as to its own past, or any care as to what shall seem, to smallness outside, its variability or inconsistency.

To die daily, that is the attribute of this mind. To be to-morrow what it was not to-day, and to admit the fact. To be firm always in work and object, to be obstinate never in question of result; to know and feel that no other criticism is, or can be, so severe and just as its own; to be ready to give up the choicest belief under conviction, but not to allow the sneers or opposition of the ignorant or half-learned to quicken doubt. To look on praise with due scepticism, and to hold to nothing wrongly because the delighted world calls *Sufficit.*

Such, according to my view, is the only frame and condition of mind by which medicine can advance in first principles of research.

I dwell on this because, if the view be true, it corrects a fundamental error in our systems everywhere. The accepted dogma is that medical science and art is to be advanced only by practice. Is that true? I entirely repudiate the dogma. That a man may practise, and practise well, and at the same time advance the art on primary principles, is true: but his two progressions must be essentially distinct. Into that which he would advance by first principles no trace of worldly life must enter: pierced in his nobler life by his lesser art he is from that moment disabled; he is no longer a unit, but a section of a crowd.

Hence I often fear lest, in the future, some of the great rewards of the deserving in medicine may come to those who live outside the pale: who, impassionless, uninfluenced by the discords within, seeing the feebleness within too shrewdly and scorning it, shall proceed alone and win, without care, by the development of truths which come only through serenity of observation.

This idea as to the men through whom science must rise direct from nature is supported by the great facts of history. It is quite

certain that in all the sciences which have risen to exactitude, the leaders of those sciences have been men who worked for exactitude, and with no other object in their advancing studies. They have sometimes been men who have followed particular occupations for the means of existence, and have turned the profits of their calling to their higher aim and work; but that higher work, itself, was pure. Thus our own Kepler, first and prince of astronomers, while he lived in his little black tent, and turned it into a camera, and anon practised medicine, was meanwhile making silently that divine, divine because pure, discovery of a common bond of suns and worlds, which won at last, led him to exclaim, in the grandeur of inspiration:—"Nothing holds me. I will indulge my sacred fury! If you forgive me, I rejoice; if you are angry, I can bear it. The die is cast. The book is written, to be read either now, or by posterity, I care not which. It may well wait a century for a reader, since God has waited six thousand years for an observer."

In like manner our Harvey, labouring out the problem of his life, dissevers the work from the routine of professional toil, and yielding to Sir Simon Baskerville the éclat of the successful practitioner, so called, is content to discover what Sir Simon, in his sublime practical wisdom, never knew, except as to him a useless mystery,—the circulation of the blood.

THE UNITY OF RESEARCH.

After the frame and constitution of mind, the order of research comes before us for contemplation. And here, the first principle that requires to be recognised is the principle of unity. At this moment, by the incoherent recognition of this unity, we are tearing our science to shreds, and hoping that, by combining the patchwork, we may produce a seamless garment. The conception is as feeble as it is motley: as the firmament is of one azure blue, as the heavens are in order, so the science that shall be perfect must be in harmony. Neither must that harmony be confined to one particular subject, it must extend to each science, and unite each one in the whole. From the rest of the science-world naught should separate the Physician and Surgeon except the art, which, in fact, is the craftwork of the science, developing it but not producing. Shall one man be a mere physiologist, another man a mere pathologist, this a diagnostic, that a therapeutist? 'Tis trifling with nature. Test the matter by comparison, There is before us a mechanism; an engine which we have not invented, and the principles of which we know not. To investigate it, to learn it in its unity, shall we divide ourselves into exclusive sections? Shall some of us take it to pieces, and figure each of its parts, its wheels and its pistons, its boiler and its condensor, its regulator and its tender? In truth they shall do much. Shall another set investigate the relations of those parts, their motion, their order of

motion, the product? they shall also do much. Shall a third set take the engine, when out of gear, and listen to its creakings and grindings, and shakings, and look into its chambers and tubes, and valves, and feel its throbbings and heavings? they shall do much. Shall a fourth division take the useless engine and examine its disabled parts in detail, and describe them? they shall do much. Lastly, shall a set treating badly working engines, try to restore them by casting various fuels into the furnace, or by letting off steam, or cutting off excrescences, or patching up holes? they too shall do much, and, in mere handicraft, they shall perchance sometimes do a vast deal. In a word, all shall do well who labour in their respective callings thus far, and the division of labour shall be true, and the results natural, within their legitimate bounds.

But, if thus dividing ourselves into varied labour, we allow the work we take in hand to isolate us from all the other work: if we make one work predominate, if we give fashion to one department, and hold that up as the *beau idéal* of our study: if we make our divisions play the role of independent centres, then we advance not a jot, not a jot, but become the creatures, rulers or subjects, of petty sovereignties each alike, poor, proud, and powerless; and, however so often the engine comes before us, sound or unsound, though it be before us under infinite variety of form every minute of our lives, and in a sense be the perpetual study of our lives, yet shall we, by the course we take, gain no more knowledge of the principle of the engine than the simple savage who looks at it for the first time with combined wonder, fear, admiration, and instinctive desire to know.

But, in physic, this divisional method of research, this centrifugal disintegration, is so much the passion that no man can be held learned who does not follow it. A man may be a great pathologist, a great physiologist, a great anatomist, a great diagnostician, a great microscopist, but a great physician he must not be; and after isolating his own greatness as widely as possible from other greatnesses, and after what he calls carving out for himself a speciality, even in his own department, he will be content, flying under the centrifugal propulsion out of nature altogether, content to tell you that it is absurd, as indeed I fear it is for him, to try to master any subject save his own.

In the behests of science, in the interests of humanity, this centrifugal training, and cultivation, must really cease if we, as a body, would stand a power: it is landing us breathless, companionless, naked, on the the shores of folly, there to set up a squalid hut and think ourselves kings. When a man, led by this propulsion, prides himself as I have heard him pride himself, and his friends for him, that, pursuing his speciality with almost supernatural vigour, he has made so many thousands of minute dissections and measurements of one particular organ of the animal body, I may laugh at the conceit of the individual, but I must weep if I contemplate, solemnly, the terrible and chaotic imbecility of a system which allows such lost labour to pass

for great labour, and which cheers the loss. If so simple a thing as a steam engine could never be learned off, as an engine, by such form of study, nor by any number of such isolated studies, how can it be expected that the unity of the animal machine can be advanced by research, in its case so infinitely less efficient?

I speak thus for the argument of science, I speak feebly echoing the voice which proclaims everywhere the unity of nature, and the All-creative Intellect. But I am not unconscious that an argument from another side may be used against me, and which, on the principle of every man for himself and heaven for us all, may be potently wielded. It may be urged that medical art and science, themselves of the earth earthy, must move with the earth as it is; that external influences, apart from them truly, but within the sphere of attraction, must tell upon them; and that, to succeed, they must conform to what may even be the prejudices of mankind. This stated, as a primary, it may further be urged that the whole tendency of the present day is to division of labour; that there is an earnest belief, fair or false it matters not, in favour of such a division, and that medicine to thrive must run in the current with the rest, and even at the risk of scientific dissolution must divide! divide! divide! Is it indeed so? Grant it—and medicine is in fragments which rub together, make noise, crash, and fall even to the lowest depth. Grant it—and where shall we limit the disintegration. Grant it, and how shall the world put on it any limitation, and why shall there not be as many classes of healers as there are organs, each healer having status according to the vital importance of the organ he treats. For my part, in humiliation I admit the existence of this theory, but in the face of truth I deny the necessity for it, or the wisdom of it. In thus bending to the sordid gravitation of the earth, there is a deformity in our art which except in Egypt, when she was sinking, has seen no light until the last thirty or forty fleeting years. In these years the Esculapian, forgetting his nobler part, has degraded himself to a common level, when he might have stood, in the earnest consciousness of his strength, above the level, and a first power in the land. Where at the present time in this country, and in physic, is to be found the type of Richard Mead, who, in his palace, where little children now pour forth their touching woes, could command the friendship of every illustrious man who visited our shores. Where is now the Physician who dare say to the Prime Minister of England, liberate a just and upright man from the durance of the political prison, or my skill is in safe keeping from your frailty? Where is the representative of Haller who shall claim, and claim to win, an equal place with the princes of philosophy? Where are the great teachers of Leyden, Padua and London? Where are the men, who like Harvey and Lower, in days, when differences of rank were far more keenly appreciated, could call royal pupils to their noble demonstrations? Alas, I know not. I know only of a profession sinking fast its art into its trade, and in some and many instances descending even to

the speculative tricks of the gambler, and to their inevitable consequences, loss of wealth, loss of mental health, and unmitigated despair.

Facilis descensus Averni, and never so facile as when the great descend.

That some one, if not I, should speak thus earnestly against the centrifugal rending of medicine is the more necessary because of the risks of delay. It cannot be concealed that one generation imbued with a particular conceit, if it retain it to the last, passes it to the next generation with increasing force, and that many, indeed all, of the deepest sectional delusions have their root in the idola of descended usage and form. A false belief, thus seated, evolves a practice of overwhelming power, because, despite the most cogent reasons against it, suggested and proved by better knowledge, men are afraid to question it or leave it. Thus, our forefathers in physic, by their too rigid adherence to wholesale blood-letting against reason, allowed even the meanest of their enemies to teach them; as, in like manner, our modern statesmen are being slowly taught, by the youngest of States, the natural truths—that life cannot be saved by taking it, that familiarity with violent death must make such death less hideous, and that the majesty of the law, which should partake of the majesty of the Eternal, can never be usefully sent forth, bottled up in the brandy-reeking pathology and offensive rags of a cowardly, concentrated and paid murderer.

With some classes, the retention of a prejudice engrafted of old, and passing through many generations, may be of service, in that it may ensure to them the praise and confidence of a world able to judge of the merits of a case or sentiment. Of the priest, the barrister, or the politician, the world may have a standard of judgment, and that judgment may have soundness in it resting upon a correct understanding of a necessity or a talent. But of us, out of our moral and social conditions, the world has no standard that is worth its possession or our appreciation. Our licenses to heal are its only safeguards, and these are governed by our own laws and opinions. In our practice we hear ourselves offensively criticised to-day, and to-morrow as offensively lauded: but by those of us who are serious in our work, the praise and the blame are measured alike, because we know that our self-conscious mistakes are as often falsely admired as our self-conscious triumphs are falsely misrepresented. Who of us, that is observant, does not constantly witness the conceit that the last physician called in to give an opinion is the best in the eyes of the looker-on? Yet who of us, that is honest, does not feel that the conceit is childish, and, by common understanding, treat it for precisely what it is worth and no more?

But for these very reasons, for the reason of the helplessness of the world in judging of us, and the security of our own isolated position, ought we to guard the more zealously the unity of our science, and endeavour to become, not many men treading diverse

paths, and aping before universal ignorance superiorities which we cannot substantiate before ourselves, but as one man treading the same path, and striving for such union of power as shall make all who will it the best in all places, and in all time. Above every thing let us not permit a future Le Clerc, Sprengel, or Freind to say of us, that in an age, crowded with every advantage, we forgot Hippocrates, and, bitten by the practices of inferior minds, turned our Temple into a market-place, each man with his own stall, and no stall with anything upon it that the historian cares to discover.

By what steps shall we then progress towards that unity to which it is essential to aspire, towards that high standard of producing and fruitful research which shall put our work amongst the philosophies, and give us command and power. Proceeding to answer this question from its negative side, let me at once express an entire disbelief of the utility of spending time in putting down what is called quackery. Of course I do not mean that men who cultivate scientific medicine, should associate with men of quackish mind and instinct, or that it is bad for us to do as we usually do, scent out with uncommon sharpness, all such representatives of the rat and weasel type, and send them to perpetual Coventry, with such marks on them, as shall fix their positions and characters too firmly to admit of mistake. But I mean that it is in vain to enter into waste of controversy with systematized quackery, because, if we can make our science pure. there could be no quackery in existence, while there will be quackery so long as the science is impure. We see that in astronomy there are no quacks, that in mathematical science there are no quacks, that amongst skilled artisans there are no quacks: and, turning to our own world, we know that even with us some parts of our field are entirely free of quacks. Who can find me a quack anatomist? Mark! as surgery has become more precise, how in surgery the quack has slunk aside. Where now is the quack woman who would venture, as in the time of good old Daniel Turner, to plunge a needle into the eye-ball to extract opaque bodies from its chambers. These things have passed away, and so shall all quackeries pass as the certain takes the place of the doubtful or obscure. Respecting blatant quackery, out of our sphere, I think we have every reason to be satisfied with its rapid decline; it has virtually ceased as a distinct and recognized trade, and in that character needs no more punishment. In our ranks, though it hides still, and, I veritably believe exists often of necessity in the natural constitution of those who exhibit it, in men of small brain and cold blood, it is growing more harmless day by day under the influence of exposure, and its own idiotic feebleness.

Leaving then the negative side of the question, let us consider in what way our lines of research shall so be carried out that our united forces shall be brought into full and combined action.

And first, it appears to me, that such weakness as we show lies amongst those of us who represent the actual living fact of physic:

lies in this, that we are walking separated from each other, on lines parallel so that we do not meet, and so broadly apart that we do not even hail each other. We are as rays of light entering transparency and yet not combined to form a focus, or if combined, making no picture of clear definition. If this be so, then the first step in our research must come from the active members of the profession, and must show itself by a resolution to arrest the present centrifugal aberration, to concentrate the forces of the science, to consolidate its basis.

In this sense, it is of first importance to forbid at once and for good the centrifugal mania of instituting a separate society for every artificially divided branch of medicine, and to prevent the molecular disintegration of the grand old Republic which our fathers left us to foster, to strengthen, to hold, to beautify, but never to dissever. It is not too late to amend this error. Let there be as many societies as there are tens to form them, if they all meet in the unity of physic; but the crash into sects, can no one stop that? Is there in medicine, and in this country, no central body that will attract the fragments and save anarchy? What should we say if the astronomers divided themselves into the telescopical society, the air-pump society, the solar society, the lunar society, the planetary society, the saturn's ring society, the asteroid society, the fixed star society, the comet and meteor society, the star spectrum analysis society, the worlds-on-fire society; to say nothing of the Keplerian, the Newtonian, the Halleyan; or the zodiacal, ecliptic, or orbital societies? What should we say? Why that the astronomers were on their last wings—a mad crew, splitting up as best they could the compact universe, and the universal harmonies, for gross imaginings, with as little compunction as a printer who distributes the type of a great book, and making themselves as loose as sand. I trust that whatever may be the power of this St. Andrews Association, it will keep to the unities or dissolve itself.

The object of all societies, in short, should be simple union of men together for a common purpose, with independence of individual action and entire freedom from all that shall tend to specialize either men or things. When a man enters an arena knowing that he will be allowed only to stand on one leg, he may, by the rehearsing he has been guilty of, and by the advantage of the trained and prepared tastes of the audience, make a very creditable display. But what belongs to the display or what comes of it? When a man, bound to a special society, is teamed with another man for a sub-special purpose, he may pull very hard and do all the work, or he may not pull at all; but in neither case will he be quite satisfied, and in neither case will the work have the breadth and touch and colour of one skilful hand. Indeed, so formed are we by nature to differ, that the evolution of a single harmonious thought by two minds is a physical accident. The early Mythology, which gave

to many gods the construction of the universe, was not intrinsically weaker, nor falser, nor more mischievous to the conception of order. Let us then, again, as an association, respect in the fullest degree the independency of members. Let every man be a law unto himself. Let us seize, as truths, from this our first day's birth, that all true progress is the result of individual labour, that genius has no double and bears no double yoking; but that, fastened in their independency by the social ties of friendship, and sharpened by the criticism of other minds, the units of the Society may each become so strong that their very individuality shall be their safest bond, and their independent strength the best surety of their united endurance.

UNITY OF PRACTICE.

To secure a sound method of research in medicine, unity of practice and of observation over the whole field of disease, is another essential. I am quite free to admit that men have certain differences of capacity and feeling which education cannot altogether equalize; and I also see that, in our great work, there are practically a few grand parts which though not altogether, nor indeed at all disconnected, do, nevertheless, allow scope for certain differences of capacity and power. The governance of the hand and the eye for precise co-operation is natural in some men, and when this gift is combined with a firm heart, the man, owning all, stands forward pre-eminently qualified for the handicraft of Physic. He is, by nature, a Surgeon, and although he may likewise be a good Physician, he will soon be detected as having the mechanical art at his fingers' ends. There are other men who are naturally endowed with perceptive knowledge of habits, tastes, and feelings, mental or physical. These men are, by nature, Physicians, and though they may likewise become good Surgeons, they are soon read off and placed by their compeers in their true positions.

Corresponding with these natural qualities of professors of our art, there are tracks in the art itself for their peculiar skill, and the general dividing of medicine into the medical and the surgical, is probably a sound practice, resting on necessity and on natural law. A plea also may be put in with fairness, founded exclusively however on artificial necessity, peculiar to the age and civilization, for the separation of the Physicians into those who treat physical, and those who treat mental disease. But beyond these general divisions there can be no rending for anything but evil. Separate an organ from an organism, and the organ no longer belongs to the organism. Make diseases isolations, and you make them entities, to be treated as such. What child's play! If the theory, hapless, were true, what hope were there for advance in knowledge of disease or its management? Entities, how many are you? Physicians, on what

is your particular knowledge of your particular entity based? Can you separate one of your entities or find an exclusive seat for it in the vile body? Well, if you can, then has your labour not yet commenced, for entities never die, and although you may live long to study yours, and to make the description of it sensational, and to talk to it and get many fees for the talking, which fees shall be, after all, the best of the intercourse, yet shall the entity remain unexorcised, and lively as ever, when you are very quiet indeed.

The entity doctrine in physic, having its origin in the dreams of Van Helmont and Stahl, dreams grossly perverted by their followers, and carried down to this day in reality, although disguised in some of the stolen clothes of science, is the curse of our profession. When the public come to form so low an estimate of an individual who should rank with the philosopher, as to believe that his knowledge is confined to one so-called disease, or to one organ, the beauty and the nobility of medicine, as a system, are for the moment gone; the great philosophy which the ancient poets compared to the sun, and consigned symbolically to Apollo, is eclipsed. And when he, who should be the philosopher, is willing to bow to a mean estimate and to say, "Your servant, sir," to the stupid who comes to him because he is the fountain head of knowlege on the kidney, or the liver, or the toe nail, or the muscles and sinews as distinguished from the bones and the joints, then is the eclipse dense of dense. The darkness may be felt.

Nothing, but a sense of an imperative duty, could warrant these sayings. Nothing, except the deepest love for medicine, could embolden me to say them. But I am tired of seeing physic sneered at as the least exact of all human things; I am wearied with the sound of the earth bells tolling our frailties; and I am saddened to recognize that the profession, blind to its interests, allows an universal specialism to nourish and force an almost universal scepticism.

THE UNITY OF EDUCATION.

To sustain the principle of unity of research in medicine, a reform of the most sweeping character is required in the matter of medical education. The young mind, brought for the first time into connection with its older life, takes up at once the impressions by which it shall afterwards be mostly guided and ruled. What then sees the student, as he enters medicine? What does he hear? What does he grasp? He sees a mirage, he hears a Babel, he grasps water.

What of the hundred things set before him to study shall he study first: who of all those debaters, for now there are no teachers, shall he believe: to what tenet, to what current shall he trust his future? Poor Student! thou hast many masters, but no master; and now thy masters, abusing with loud sounds, the scissor workers of physic out of the privileged house which thou walkest in, are so

far imitating them as to snip up for thee sundry similar patterns, even in that guarded house, which thou must also learn. I do not wish to over praise the old days of apprenticeship, and seven years' surgery; but beshrew me, Student, skipping about in that big asylum, now in this lecture room half asleep, and anon in that lecture room very much too wide awake; this hour pouring out thy broken ideas on that operation, criticising Peter thy master, at the expense of Paul thy master; and finishing up the day with a discussion at a society's meeting, on a subject the alphabet of which is not in thy possession: I say, Student, perchance thou wert better in the seven years' surgery after all, and vastly better if there thou hadst over thee one good master, who would give thee all he has except experience, and years, and ripeness of thought, and thine own soul.

Unity in medical teaching is a call urgent and unmistakable. As many schools as you may find teachers for them, as much rivalry as you please, but unity in the school, unity of thought, unity of word, unity of expectation and object, these are the urgent demands of the day and the hour.

Neither are the demands on the side, and in the interests, of the taught alone, they extend to the teacher. The art, the skill, the endurance, the fame of the teacher are all in rapid decline. On the one hand, so keen is the competition for practice, so much richer are its rewards; on the other hand, so widely distributed are the responsibilities of the teacher, and so small is the return for skilled and profound teaching, that there is no practical competition whatever for professorial fame. Before he is himself schooled in schooling, your modern Lecturer is out of the chair and into the chariot: and thus we are accustomed to hear of the various schools that "the school is of no moment," that "one school is as good as another," and that every thing in the way of success rests with the Student. The argument is irresistible, but is it good that it should be irresistible? Is there nothing for the student to learn in the patience, the impassion, the solidity, the labour, the zeal, the devotion of a great teacher? Was Fabricius nothing to Harvey, was Beddoes nothing to Davy, was William Hunter nothing to Brother John? Let the eloquence, the genius, the force of the great teachers of old pass into the grave, and the dead have buried their dead, and the living have no more life.

It is worthy of our consideration as an associated body, whether we cannot ultimately, through our University, resuscitate the greatness of medical learning; resuscitate its poetry, its philosophy, and to the ingenuous mind, its practical utility. St. Andrews hath not always slept, but hath sometimes done the waking work of centuries in a few short years. If she should wake again, should she not wake as one refreshed by long slumber, charged with new force, determined to prove her vitality, and to transcend her ancient fame?

THE DIRECTION OF RESEARCH.

Presuming the centrifugal system of modern medicine be abandoned, or so modified as to lead us to some common understanding, what are the directions towards the unity of which I have spoken? The first step is to bring all minds to bear on the simple physical relations of animal force to animal matter. In animal bodies I find two different forms of matter, one I call fatty matter, the other I call albuminous. I take the fat, which is a solid, I apply to it force, by the heat of a lamp or fire, and from the solid it becomes of fluid form. I take the albumen, which is before me liquid in water, I apply to it the same force, and lo! the liquid becomes solid. I take a dead animal and expose it, directly after death, to a temperature from five to six degrees above freezing point, and its fat solidifies, but its muscles remain flaccid. I take another animal recently dead and place it on a sand bath at 110°, and its fatty matter remains fluid, but its muscles contract and fix in the firmest rigidity. I take blood, newly drawn and fluid, I place it below 44°, and it holds a fluid: I raise the temperature so little as ten degrees, and it yields me a firm coagulum. I take gelatine, solid and like to dried coagulated fibrine, and treat it with heated water and it dissolves, into what resembles water in tenuity, to solidify as it cools. What mean these opposite effects of the same force? How does the force bind one thing and loosen the other? Why in the organs of active locomotion have we only matter that is put into contraction by force, while in the nerve tissue we have compound matter, one part of which is solidified, the other fluidified, by force? How is force laid up in nervous matter? Why in one case is it fatally poured out, and in another case fatally suppressed? These, and many more questions of the same caste, lie at the root of the explanation of living phenomena healthy and diseased; nor can a step be made in scientific healing until they are answered. With the relations of force to matter unknown, the morbid dissecter, like the grave-stone cutter, cuts only to record; the pathologist is an abstraction, and the therapeutist a sceptical believer, who fears to trust, dares not mistrust, and relies on an experience which varies like the deceitful sea.

The simple relations of force to matter understood, all that requires to be known by the physician, in every department, would proceed as light proceeds from the sun. He would see phenomena as natural and sequential, and what now appears to him out of the order of nature and as disease, so named in abhorrence or disgust, would be found to be in the order and to be preventable, remediable or removable, according to the necessities of order. Then would he see, and know, the limitations of his power, nor waste his time in what, for aught he knows now, may be vain efforts to resist the inevitable. Then would he know the full extent of his influence, and gather, I predict, such marvels of know-

ledge that the crude beliefs of the wondering world should be shaken to their centre.

But towards this end the cry is again for the unity of research. As none but the "God can love from whole to parts," as no service, nor labour, nor work of man hath ever descended from the whole to the parts, but hath always proceeded from the individual to the whole, when success hath attended it; so, in medicine, we must move from the primary fact of life to the whole manifestation.

And here will be the point of the conversion of medicine from dissolute and prodigal weakness into absolute and united strength, that her wandering children, ceasing, in sheer and useless weariness from their isolated tasks, and called by the trumpet-voice of some great leader, shall lend themselves manfully to the discovery of the primary, and, from that discovery, march onwards in the consciousness that the accepted is the proved, and that what is not proved is not known.

In the midst of much confusion and, as I have shown, of fearful centrifugal rending, there stand forth two facts of promise.

I notice, first, that there are one or two stranger voices whose words are reaching us. Amongst these the voice of Thomas Graham, Master of her Majesty's Mint, is specially attracting us. His language is strange to many, but there are single notes, and I had almost said harmonies, whioh come with power upon the mind. He will be listened to more.

I notice, secondly, that there is almost a thankful willingness to resign Abra-ca-da-bra! We had for a long time a book of common forms; a Latin book, in Rose, Shamrock, and Thistle editions, full of tales

"Of herbs, plants, flowers, and their true qualities,"

writ in Latin. At last the strongest man in the practical reform of our craft in our time—the Cromwell of physic—condemned the book and its mysteries, and now it floats, in simple English dress, in time to make its bow and retire altogether. Before I name the book I must, in parenthesis, say who this strong man was. It was the man who, opening the locked doors of hospitals, nailed them open, and who left behind him a work which—though its blasts and its hurricanes are subdued, though its noble Saxon tongue, ringing like the hammer of Thor on an anvil of silver, hath caught the Norman lisp, and though the hand of the poet, born not made, moves in it no more—is, notwithstanding, under the momentum of his genius, still one of the most powerful class-journals in the world. The man I mean was the late Mr. Wakley.

As for the book which he condemned, and which flourishes in this day as the "British Pharmacopeia," though the reform in it is not stout winning, it is a good giving-up of dead weight, and, amidst an ocean of discouragement, is a faint but hopeful sign of advancement. It is beyond what it seems. It is leading us to ask

whether simplification cannot be carried out further, and whether investigation as to remedies cannot be rendered more precise; it affords scope for the introduction of principles in therapeutics, and, binding us by a more correct nomenclature with advanced chemistry, it connects us closely with one section at least of the more accurate sciences.

THE ULTIMATE OBJECT.

Hitherto I have spoken of research in medicine as connected with our present interests and aspirations; but the grand, the overpowering object of such research rests, after all, in the hopes of the future. Who fails to realize this truth hath nor part nor lot in victory. If the unity of belief, that we live for the future, exist not amongst us, but be left to the prescience of individual minds, then is unity of labour still far, far removed, and the mourning of the world wailingly prolonged. Small we may be, but if the world wait for our combined action, then ought we to be great. And who dares doubt that the world doth wait even for us?

The glories of that happier time, for which all creation yearns, what are they but the glories of life relieved from pain, from want, from care? Are not these reliefs our duties? Is it not our office to be the first of men to pluck the curse of pain from the whole earth? Is it not our office to economize the gifts of nature, and lend her wealth to health? Is it not our office to soothe the troubled mind and bring the disturbed brain to equilibrium of power? If these be not our offices, who are the blessed that claim them? If they be—then the sweetest singer of Israel, telling of the time when "There shall be no more thence an infant of days, nor an old man that hath not filled his days;" and the Roman poet, singing the

> "*Ultima Cumæi venit jam carminis ætas*
> *Magnus ab integro sæclorum nascitur ordo;*"

and the mighty apostle, thundering through the ages, "The last enemy that shall be destroyed is death."—Then these are our prophets proclaiming to us our mission, and assuring us that, if the mission be faithful and their prophetic visions true, we, in life or in death, shall be as kings in the kingdom of Our Father.

REPORT ON OZONE.

By H. Day, M.D., M.R.C.P., Physician to the Stafford County Infirmary.

INTRODUCTION.

Since the year 1840, when Schönbein commenced to place before the world his researches on "Electrified Air," "Ozone," or as it has subsequently been called "Allotropic Oxygen," no larger subject than this has engaged the attention of scientific men.

The subject has attracted the attention of three classes of observers—the Physicists, the Chemists, and the Physicians.

The Physicists have been struck by the bearing of the question in relation to the general phenomena of the universe—by the changes, as it would seem, which mere matter may be made to undergo under the influence of force: for, and from, this reason too, their minds have been brought to the consideration of analogous phenomena observed in the transmutations of Phosphorus under the influence of heat, and some kind of countenance has been given to what may, perhaps, be called the ancient-modern notion which the alchemists believed, and a Faraday did *not* disbelieve, that a day will come when elementary forms of matter will be reduced to a few simple types, if not to a single one.

To the Chemists the subject has been of interest, in that they have found a new body to occupy their attention, new compounds springing from it, new views respecting the molecular condition of elements, new speculation as to combinations, and the expression of combinations, by symbols and equivalents.

To the Physicians Ozone has afforded a wider—I had almost said a wilder—field: they have believed they saw in it a cause of disease—a remedy for disease—a preventive of disease; it has been obtrusive—it has been perplexing—it has been a subject of vast erudition, and yet it is, up to this day, a subject of intense doubt. Whilst some hold it almost in wonder, others are heretical enough to dispute its existence altogether, the majority hesitating and asking for more light.

During the time I have been writing this paper, all these phases have been presented to me, and it is a singular circumstance, but quite true, that two brothers, both physicians of eminence, one residing in this Country, and the other at the Antipodes, and both connected by the common tie of our University, are as widely separated in opinion on the subject of Ozone, as they are in the geographical limits of the world.

How then shall I proceed to speak in this assembly? I who have no pretence to be a judge of matters of such profound importance! I know of but one course, that is, to proceed as I have learned, determined, at the onset, not to embarrass myself with any anxieties as to who is right and who is wrong.

HISTORY.

That we may be carried on smoothly from one point of our study to another, let me briefly relate the circumstances connected with what may be considered to be the discovery of the nature of "Ozone."

The earliest electricians appear to have formed an idea that common air did not admit of electrization; for instance, Benjamin Franklin, whose researches in electricity are second to none, says, in a letter addressed by him to Peter Collison, F.R.S., of London, in 1749: "Air is an electric *per se*, and, when dry, will not "conduct the electrical force, it will neither receive it nor give it "to other bodies; otherwise, no body, surrounded by air, could be "electrified positively and negatively; for" — he argues — "should it be attempted positively, the air would immediately "take away the surplus; or negatively, the air would supply "what was wanting,"

In the main, there can be no doubt Franklin was correct: it is clear that air can never be so electrified as to become either a conductor of electric currents, or an intense conductor of electrical force; if it could, what Franklin said would be true, the air would be the *prime* conductor and the *prime* holder of all electrical force: it would neither insulate the wire of the telegraph, nor permit the largest machine to charge the "Leyden Jar." But the generalization of Franklin was rather too sweeping notwithstanding.

It will be remembered that he wrote the above before the time of Priestley, and that in speaking of Air, he included under that term what he believed to be an elementary body, and, so far was he right, that, even now, with our knowledge of the compound nature of air, and the capacity of one of its elements—oxygen—to be influenced by electricity, and with electrical apparatus of the most powerful kind at our command, we can only affect one part in twelve of that oxygen which forms only one fifth of the atmospheric scene.

The discovery of Oxygen by Priestley led, after a few years, to the revision of the Franklinian hypothesis as to the absolute incapability of the air to be influenced by electricity.

In 1789, Van Marum, an electrician of Holland, practising on the Vital Air of Priestley—or, as it has since been named, "Oxygen"—discovered that, after electric sparks had been passed through this gas, a particular odour was developed, which he spoke of as "an electrical odour," and which represents what is now called, on the same nomenclature, but from a Greek derivation —"Ozone."

Towards the beginning of the present century, Cavallo seems to have made a further step, for he observed that a peculiar condition of air was produced when passing through it electric sparks, which condition he designated the "Aura Electrica." Cavallo further observed that this electrified air had a purifying effect on decomposing organic matter, and he therefore had it applied, as he thought with good effect, to fœtid ulcers of the human subject.

One of the discoveries about this period, having an indirect bearing on the subject before us, deserves attention, the more so, because it independently confirms some more recent observations made by two of the latest experimentalists. The discovery I refer to was made by Aldini, the nephew of Galvani, and Professor of Experimental Philosophy in the University of Bologna, and who became celebrated in this country by his remarkable experiments with Galvanism on the bodies of executed criminals. Aldini observed that when, by means of a metallic point, he electrified the interior of a glass jar, which he inverted and placed over a plate of metal so as to form, what he called, an "*Insulated plenum*," the water in the glass rose several lines in a very short time: afterwards he repeated the experiment, using mercury instead of water, when he noticed the same occurrence, but in a less marked degree.

In these experiments, evidently without knowing the meaning of the facts he had observed, Aldini saw the condensation of Oxygen under the influence of Electricity.

Nearly forty years now elapsed before any further notable advance was made, then Schönbein, the late well known Physical Philosopher, commenced his famous researches; and taking the view that the electrical modifications of air rested solely on modifications of the oxygen, proceeded at last to theorize that oxygen itself presents three distinct conditions: the first, a negative polar condition, which he termed "Ozone;" the second, an opposite condition, which he named "Antozone;" and the third, a neutral state, resulting from the union of the two.

It was fortunate for Schönbein that his researches very early obtained the notice of Professor Michael Faraday who, although he attempted but little in original research in this one direction, made

the scientific public, by his brilliant lectures, most familiar with what had been done, and excited many observers to prolonged and careful study.

The presence of a peculiar body, possessing special properties, once established by Schönbein, its nature became actively canvassed, and our countryman, Williamson, started the theory that Ozone was a ter-oxide of hydrogen: this theory, based on the supposition that water was always produced when Ozone was removed, assumed the absolute necessity for the presence of hydrogen for the production of Ozone. It remained for Andrews and Tait to dispose of this view, by proving that when hydrogen was entirely excluded, and absolute oxygen made to receive the electric spark, the change of oxygen into Ozone, to the extreme extent of one part in twelve, could be demonstratively established. The same observers further proved by after experiment, that, in this process of change, there is condensation of the oxygen and reduction of volume. Finally, they proved that when mercury was exposed to ozonized oxygen, the Ozone disappeared, the volume of gas remaining precisely the same.

The last theory as to the nature of Ozone, and which brings us up to our present knowledge of this curious gas, has been advanced by Dr. Odling, and confirmed, experimentally, by M. Soret.

The facts bearing on this point have been very ably epitomized in a leading article in the *Medical Times and Gazette* of October 5, in the present year, in the following words:—

"It is now conceded, by nearly all chemists, that each molecule "of oxygen in the free state consists of two atoms; that, in fact, "the true formula for free oxygen is O_2. Odling suggested that "the formation of Ozone might really be the condensation of "another atom of oxygen into each molecule, and that the formula "for Ozone might therefore be O_3, and its density one half greater "than oxygen. When 100 volumes of oxygen were reduced by "Ozonization to 92, it might be supposed that 8 volumes of "oxygen, combined with 16 volumes, and produced 16 volumes of "Ozone, the change might be represented in this way:—

$$4O_2 + 8O_2 = 8O_3$$

"A molecule of Ozone O_3, occupying the same volume as a mole-"cule of Oxygen O_2. The absorption of the Ozone by mercury "iodine, etcetera, might really be only the removal of the third "atom of oxygen, which would, of course, leave the volume "unaltered:—

$$\underset{\text{10 volumes.}}{8O_3} + 8Hg = \underset{\text{16 volumes.}}{8O_2} + HgO$$

"The same view would account for the mutual reduction which

" Ozone and peroxide of hydrogen exercise upon one another, and, " in fact, for all known reactions of Ozone.

Ozone.		Peroxide of Hydrogen.		Oxygen.		Water.
O_3	+	H^2	O_2	= $2O_2$	+	H_2O

" This beautiful hypothesis, however, must have remained a mere " hypothesis, but for the remarkable experimental verification " which it has received from the hands of M. Soret. We have " seen that all ordinary substances are only capable of removing " one atom of oxygen from each molecule of Ozone ; but M. Soret " has at length found a body—oil of turpentine—which absorbs " the whole molecule, the whole three atoms of oxygen. To take " our previous illustration, If the 92 volumes of ozonized oxygen " were treated with oil of turpentine, a dense white cloud would " appear, the Ozone would disappear, but instead of the volume " remaining the same, it would contract to 76 volumes, the 16 " O_3 having been removed bodily instead of being merely reduced " to 16 O_2.

" This experiment (the writer goes on to say) seems to place " the matter beyond a doubt, and instead of the mass of hypotheses " which so lately reigned, we have now a simple, beautiful, and " coherent theory which affords an intelligible explanation of " known facts. It is the more to be rejoiced at, since the " importance of Ozone in art, as well as in nature, seems to be " rapidly developing, and it is impossible to say how high that " importance may rise."

One or two more points, and my narrative respecting the nature of Ozone must be concluded.

In the first place, I would remark that the hypothesis of Schönbein respecting the three distinct states of oxygen has fallen into disfavour ; secondly, that the manifestations of Ozone, as a distinct body, can only be demonstrated within certain limited degrees of temperature : at what reduced degrees of temperature Ozone may become manifested is a question as yet unsolved, but, that it disappears at an elevated temperature, admits of direct proof. In common language, then, Ozone is destroyed by heat ; but it is possibly more correct to say, that it loses its character, or certain of its special characters. Why this should be so is altogether unexplained, except on the supposition of a transmutation of force.

MODE OF PRODUCTION.

Several means exist for the production of Ozone besides those natural means, whatever they may be, which bring it into the air without our calling for it.

Amongst the several artificial modes, the first to be mentioned should be the original one of passing the sparks of an ordinary

frictional Electric machine through the air, or, through oxygen: an improvement on this method has been accomplished by the use of Ruhmkorff's Induction Coil, which machine we have before us. When sparks are passed between the electrodes of this machine, the air is ozonized; but in order to collect the air thus ozonized, it is necessary to call in the aid of an additional apparatus, which is called an "Ozone Generator." Siemen was the first to use the Generator; his instrument, of which I will endeavour to draw a rough outline, consists of two cylinders of glass covered, or rather, partially covered, with tin foil, one cylinder being enclosed within the other, there being a space of about half an inch between them. A wire from one of the electrodes of the coil passes to the tin foil of the inner cylinder, and the wire from the other electrode passes to the foil of the outer cylinder. When the coil is in action, electric sparks are freely discharged between the two cylinders, and when a current of air is driven, by a pair of bellows, through the space in which the discharge takes place, the air is rapidly ozonized. An improvement on Siemen's apparatus has been constructed by Mr. Apps, the Philosophical Instrument Maker in the Strand: a sectional drawing of it is here, and the instrument itself is on the table. If now we set the coil in action, and drive air, with the bellows, through the "Ozone Generator," collecting the ozonized air in bottles placed in the pneumatic trough, we can speedily fill several bottles with air saturated with Ozone,

The odour of Ozone may very readily be detected for some time in these bottles. Workers in Ozone will forgive me for this simple experimental illustration; but, as this is the easiest way for producing Ozone, and as it is my wish to familiarize every one with the process, it could not very well be left out. For all experimental researches, this is by far the best and readiest mode of production.

Another method consists in passing air over Phosphorus partly immersed in water: a simple Ozone maker of this kind was invented by the late Dr. Barker, of Bedford; and another, I believe, by our colleague, Dr. Moffat, of Hawarden, whose researches on the relations of Ozone to disease are so widely known.

There is also another mode of obtaining Ozone, not very applicable, but forming a pretty experiment: it consists in diffusing the vapour of Ether through a bell-shaped jar, and then inserting in the vapour a heated glass rod.

The last suggested method is one by M. Morin: it consists in dispersing a current of water in the pulverized form, in other words, in the form of spray.

These are some of the principal artificial means for obtaining Ozone: of the manner in which it is produced in nature, various theories have been promulgated. Dovè, so well known for his

theories on the laws of storms, thinks that Ozone is generated in the higher currents in the atmosphere, in fact, during the condensation, in their polar course, of the heated equatorial currents. Moffat believes he has traced the Ozonic atmospheric periods to particular winds, and also to certain phosphorescent states. Lowe holds that the apparent varying intensity of Ozone may possibly be attributed to circumstances operating for, or against, its detection; thus, an increase of temperature from increased chemical action, would show an increase—a greater velocity of air, an increase, of moisture up to saturation, after which a minimum will result. At his (*i.e.* Lowe's) observatory at Highfield House, near Nottingham, most Ozone occurs during the prevalence of a south wind, and least with a north-east wind, the maximum being attained when the barometer is at its lowest readings, and the minimum when at its highest; this, he thinks, may be simply owing to the increased velocity of a south wind over a north-east wind, its increased temperature and its moisture. Others have argued that Ozone is the result of electrical discharges in the air, and several have assigned to vegetation the generation of this gas, amongst these last I may name Dr. Daubeney, of Oxford. Dr. Daubeney, judging from the depth of coloration produced upon Schönbein's papers by exposure to the open air, as observed during eight months, inferred that the quantity of Ozone at Torquay was much greater, on the average, during the prevalence of winds from the sea: he also endeavoured to ascertain whether this Ozone was generated by vegetation, and, although he found that light alone coloured the paper, he was disposed to think that, after deducting this, a certain residual effect was due to the green parts of plants in generating Ozone during the day.

Presuming that the opinion of M. Morin is correct, and that Ozone can be produced by the spray of water, the prevalence of Ozone during sea breezes might possibly be traced to the action of sea-spray.

In order to ascertain whether there was any plausibility in this last named theory, I tested the effect produced by the spray of fresh water and sea water, using Richardson's spray tube for the purpose of diffusion, and directing the spray against plates of Ozone test paper.

I first used *Distilled water*, and kept up, vigorously, a continued spray upon the paper for five minutes, but without producing any change of colour that indicated the presence of Ozone.

I next tried *Rain water*, the specific gravity of which did not appear to differ materially from that of distilled water: the spray was continued for the same length of time as in the former experiment, and with a similar result.

I now used *River water*, obtained from the river Penk, the specific gravity of this was 1·003, the effect on the paper corresponded with that produced in the two former instances.

I then tried *Sea water*, having a specific gravity of 1·268, the spray was in this instance, as in the other experiments, continued for five minutes, and at the expiration of that time, I fancied there was an appreciable difference in the colour of the paper; but the alteration, if alteration it might be called, was so faint, that I thought it desirable to repeat the experiment, and, upon doing so, only obtained the same effect as that displayed with the other waters.

I next dissolved sufficient *Tidman's Sea Salt* in ordinary pump water, until it attained a specific gravity equal to the sea water: it was used in precisely the same manner as in each previous experiment, and afforded no evidence of producing an Ozonic influence on the paper. I, of course, used fresh paper for each experiment. As far as these experiments go, and taking them for just so much as they are worth, the result given was altogether negative.

As regards the general question of the natural production of Ozone, many difficulties stand in the way of its solution: so many bodies present in the air produce such similar effects: light, as Dr. Daubeney has observed, is a source of interference, and the circumstances under which the Ozone seems to appear are so disconnected and variable, that I regret to be unable to report any solid progress in this direction. When we are dealing with artificially prepared Ozone, we are treating a tangible body; we can select either atmospheric air or pure oxygen for our research—be certain that we have nothing whatever present that is extraneous—and then ozonize by one or other of the means previously described: in fact we can remove nearly all obstacles to investigation; but in dealing with air, the position is entirely different, and uncertainty, instead of certainty, meets us in every foot of our research.

TESTS.

There is, up to the present time, only one recognized test for Ozone—namely, the solution of the iodide of potassium and starch. Owing to its power as an oxidizer, Ozone acts readily on the potassium of the test compound, the potassium is thus separated from the iodine, the iodine set free, combines with the starch, forming the iodide of starch, giving a blue colour, which is the test. Test papers, consequently, have been used, the papers being saturated with a solution of iodide of potassium and starch. Three forms of test papers, thus prepared, are now in use—namely, Schönbein's, Moffat's, and Lowe's.

Schönbein's papers contain ten parts of starch and one of iodide of potassium.

Moffat's, two and a half parts of starch, and one of iodide of potassium.

Lowe's, five parts of wheat starch, and one of iodide of potassium.

Lowe has also brought out another, in which, on the recommendation of the late Dr. Dundas Thompson, fifteen grains of chalk are added to each ounce of dried starch, to prevent sourness: this precaution affords great uniformity of effect. In a further research, Lowe has used what he calls "a dry powder test," using powders of dried wheat starch combined with iodide of potassium: one part of the iodide of potassium to five of starch being the most sensitive.

I have had sent to me, within the last few days, some test papers prepared by Dr. John Day, of Geelong, Australia, who has made Ozone a special object of study, and who advocates, with great ability, the views of M. Schönbein. These test papers are remarkably sensitive, but I regret to say that I have not, at present, been made acquainted with the relative proportions of the ingredients used in their preparation: some of these papers are on the table, together with a coloured test scale.

In order to detect the presence of Ozone in the open air, Ozone test papers, or test powders, are ordinarily placed in a box, or Ozone cage. The box is coloured dark within to avoid light; but air is freely admitted. This is the apparatus employed, I believe, by all the meteorologists who supply the reports to Mr. Glaisher.

The amount of Ozone present is measured by the intensity of the colour, and is registered on Moffat's scale. There are objections to the Ozone box, which objections have been well pointed out by Mr. John Smyth: this observer takes a dissimilar view to Lowe, who thinks that the velocity of air makes a distinct difference in the registration of Ozone: he (Mr. Smyth) has devised an apparatus for securing greater uniformity of results: it consists of a large aspirator, by which air is drawn with considerable velocity through a small tube, so as to impinge, at its opening into a tube of larger dimensions, against an extended surface of test paper. By means of a cord connected with a clock, in which an extra weight is slung, a gasometer, which acts as the aspirator, is raised at an uniform velocity, and over which a pendulum gives complete control: a counterpoise is used to support the weight of the gasometer. Mr. Smyth shows, from a table of experiments made with this improved ozonometer, that there is not much difference in the quantity of sensible Ozone in two masses of air of equal volume, although they may be moving at different velocities—in different directions—and under different hygrometrical conditions. He is furthermore of opinion that the ordinary test papers do not register high enough.

Admitting, to a certain extent, the correctness of Mr. Smyth's arguments, there is a sort of fallacy in his experiments—namely, that he may be *producing* Ozone in his apparatus by drawing air

with great velocity, and much friction, through a small metallic tube.

The great objection to these tests is that discolorations are produced by other agents than Ozone, particularly by iodine, the chlorides, hydrochloric acid, nitrous and nitric acid. In the atmosphere, the last named acid from its comparatively frequent occurrence, especially after electric storms, is a serious objection.

The late Dr. Robert Dundas Thompson affirmed that even carbonic acid would affect these papers when they were very sensitive. As this latter point is of importance, I tested some highly sensitive paper with carbonic acid in its nascent form, and after it had been collected, and found that it occasioned a slight, but evident, change of colour.

It has occurred to my own mind, that possibly a delicate test for Ozone might be found by observing the effect produced by the electric spark when passed through rarified Ozonized air: for this purpose, a glass globe, such as the one we have here, should first be filled with air that is saturated with Ozone, and then be partially exhausted: through this rarified Ozonized air an electric spark should be passed, and the *colour* of the spark should be noticed, and compared with the colour of the spark when passed through rarified common air contained in the same vessel.

But this experiment should be carried further, by bringing the spectroscope into use.

As, by some, all gases are supposed to be the vapours of metals, it is feasible that the spectrum would give a different band, or bands, with rarified Ozonized air, to that or those afforded by common air, under similar circumstances and conditions. I shall report on this subject at a subsequent session.

PHYSICAL PROPERTIES OF OZONE.

Whatever views may be entertained as to the chemical nature of Ozone, it must be admitted to have certain physical properties which mark it distinctively. We have already seen from the experiments of Soret, that it is heavier than oxygen: it is also quite insoluble in water: its power is intensely increased, so that air, saturated with it, destroys dead organic matter with great rapidity: thick india-rubber tubing is quickly eaten through by it, and ammoniacal products are speedily decomposed. In contact with ammonia, as Dr. Wood first pointed out, it forms white fumes, like white fumes of chloride of ammonium, as chlorine does, producing a specific compound, or salt, an "Ozonide of Ammonium." We have proved further, that it possesses a peculiar odour: thus, in relation to weight, diffusion, chemical action and smell, it is, at all events, a distinct body; nor can, I think, its individuality be doubted by any, until these physical characteristics are explained away.

PHYSIOLOGICAL ACTION.

The physiological action of Ozone has been studied by many observers, and with singular uniformity of results. It has been tested in regard to its effects on living animals, on dead tissues, on blood, on various secretions, and on various excretions, compound and simple.

Let us first take under review the action of Ozone on living animals.

If a warm-blooded animal be placed in a glass chamber, and be subjected to a stream of Ozonized air, the oxygen of that air having been Ozonized to the 12th part, and the influence of carbonic acid being entirely excluded, special physiological phenomena are quickly displayed. The first sign, or symptom, is an irritability of the mucous surface of the nostrils, and of the conjunctivæ: there is often free secretion of saliva, and even profuse sweating in those animals that exhibit sweating: there is also thirst and dryness of the tongue and nostrils. These symptoms are succeeded by great rapidity of respiration, and soon by violent action of the heart.

When the chest is auscultated in this stage, there is always dry bronchial breathing, and a whistling sound, as in the first or preliminary stage of acute bronchitis in the human subject.

The effect of the Ozone being sustained, cough manifests itself, followed by secretion of frothy fluid from the bronchial surface: this is equivalent to the congestive stage of bronchitis. Finally, there is lividity of the skin of the nose, of the nostrils and of the lips, great coldness of the surface, gasping respiration, jactitation and death, the death being often sudden. This may be said to resemble, most perfectly, the exudative stage of bronchitis. This order of symptoms, or phenomena, as they, perhaps, had better be called, has been recognized by all experimentalists: it has been pointed out, with particular care, by our President, and my own experiments have been attended with corresponding results. There is, however, a remarkable difference in the periods of the phenomena noticed in different animals, their order, nevertheless, being maintained in each case in which they are manifested. Guinea pigs are peculiarly susceptible of the influence of Ozone: these animals die in an atmosphere saturated with Ozone, usually, in about an hour, or an hour and a quarter, presenting, with great exactitude, the order of symptoms I have detailed above. Rabbits live longer than Guinea pigs, exhaling water from the lungs much more freely, and also micturating with greater freedom. Rats die very rapidly. Mice exhibit a greater tolerance. Pigeons resist the effects of Ozone much longer than Guinea pigs, and may readily be taken out of the chamber in which they have been confined with

Guinea pigs, apparently but little inconvenienced at the period when the Guinea pigs are dead: if, when just removed, the chest is auscultated, the breathing, however, is found to be particularly sharp, dry, and cooing, the action of the heart being amazingly rapid, reaching even 240 beats in the minute, and the respiration being from 90 to 100 in the same time. Pigeons removed altogether from the Ozone, when in this condition, will frequently exhibit the third stage of obstruction to respiration, and die. Frogs seem to have the power of resisting the action of Ozone permanently, *if* they are allowed free access to water.

MORBID PHENOMENA.

The morbid appearances presented after death from Ozone in warm-blooded animals, are principally confined to the respiratory passages. If an animal be killed during the *first* stage, the bronchial membrane will be found to be dry—deeply congested at spots—the lung structure being ecchymosed, and both sides of the heart full of blood. In the *second* stage, the whole lung is congested, the bronchial surface being red, the right side of the heart engorged, the left side of the heart empty. In the *third* stage, the lungs are also intensely engorged, the bronchial surface paler and filled with frothy fluid, the right side of the heart full, the left side empty. In cases where animals are removed from the chamber at the beginning of the second stage, and, after some exposure to the air, subsequently die, the morbid anatomy is rather that of pneumonia than bronchitis: in one experiment that I performed, the stage of hepatization was so distinctly marked, that I could not have recognized it by the lung itself, from hepatization of the lung in the human subject after pneumonia.

It is, I think, worthy of remark, that very young, or very feeble, or very old animals, suffer much more readily from bronchitis produced by breathing Ozone, and succumb much more easily to its effects, than do full grown vigorous animals,

If animals die in the earlier stages of the disease produced, and especially if they die suddenly in such stages, the blood is found firmly coagulated in the heart, however quickly the body may be opened after death; but fibrinous separations never seem to have been discovered, the time for such separation having been evidently too short. Cadaveric rigidity is also, in these cases, quickly developed; but, when the symptoms run on to the stage of lividity, the blood will remain fluid for several minutes after death, and the absence of rigidity of the muscles is proportionately prolonged.

RELATION OF THE ABOVE FACTS TO CERTAIN DISEASES IN MAN.

Amongst the several, indeed numerous, surmises as to the relationship of Ozone to many diseases in the human subject, we have only one series of basic facts on which to ground any safe inference: these facts are, that the inhalation of Ozone, by inferior animals, produces symptoms analagous, if not identical, to the symptoms of catarrh, bronchitis, or even pneumonia in the human subject: hence it might be inferred that the same agent, present in the atmosphere in excess, would light up those diseases. The great question then arises whether there ever is such an excess of Ozone in the air as would be likely to create such a result: if such were the case, it would follow that man is more susceptible to the effects of Ozone than inferior animals; for, in the laboratory, and during experiments, although irritation of the air passages, and pain in the head may be occasioned, and many symptoms like nasal catarrh occur, yet, for such an effect to be produced, there must be with the man, as with the animal, an excess of Ozone quite appreciable to the ordinary senses.

When to the above facts we add the great difficulty of determining the actual existence of Ozone in ordinary atmospheric air, we must hesitate to accept any dogmatic conclusion in regard to Ozone being a natural cause of disease. There is, moreover, this important fact to bear in mind, that taking animals of the same species, and subjecting them to the same Ozonic conditions they suffer the same symptoms, although not, in each case, in the same degree perhaps. This rule should logically follow, although not necessarily to the same extent, in regard to the human family, if it, in sections or masses, were subjected to the influence of the same universal agent, which is not the fact. To put forward an analogy: If in the air, under the influence of storm, or aught else, some subtle body, such as chloroform, could become equally diffused, in quantity sufficient to influence any one individual by producing narcotism, it will not be disputed that all other individuals breathing the same air, would exhibit, more or less, the symptom of narcotism. What then is the difference, if there be any difference? The difference must lie in what may be called individual power of tolerance of an active physical agent: a tolerance not even marked, in the same individual, at all times.

Another view also presents itself to the mind: it is this—if Ozone really causes disease, it is quite certain that the tests we have are altogether inefficient to record its presence when it *is* producing disease; *or*, there is some other agency at work, which interferes with the physical facts without interfering with the determinate physical action.

Finally, it has to be considered whether, in regard to epidemics,

specially attributed to Ozone—I mean catarrhs, bronchitis, and the like—there are not other perfectly efficient causes at work to excite and create these maladies, independently of Ozone.

The recent report of the Registrar-General shows, so definitely, the influence exercised by simple variations of temperature in this respect, that really, ignoring these variations to look for more obscure causes, seems like going from the real and ascertained to the problematical, without any other reason than love of novelty.

I put forward these objections, not with the mere view of rebutting theory, but with a desire to enforce a more direct proof of theories, plausible on their surface, but unquestionably open to serious objection.

I think it *possible* that in studying Ozone, admitting all the difficulties, and determined to trace out the rigid truth, we may, in time, discover a connection between the presence of Ozone in the air, and catarrhal and bronchial epidemics: but further than this, I cannot concede the point, nor admit the smallest opening towards an argument tracing any other class of disease to atmospheric Ozone as the cause.

ACTION ON ORGANIC COMPOUNDS.

Whilst the effects produced by Ozone on a variety of organic compounds have been found, by a large body of experimentalists, to be tolerably uniform, the explanations given as to the cause or causes producing such effects have not been, in some instances, altogether in accordance with each other: but this is not more than might naturally be expected to be the case, when it is remembered that, up to the present time, different theories have been started and supported as to the true nature of the body experimentalized with.

Blood absorbs Ozone with great rapidity, and oxygen is at once freely liberated: this capability of liberating oxygen has been said to reside principally in the red globules, from the circumstance that, when freed from fibrine and albumen, these bodies produce this identical effect: further, when Ozone is passed through freshly drawn blood, the red corpuscles lose their colour, and, in the process, are destroyed. M. Schönbein adduces, as an additional proof, the circumstance that such decolorized blood is no longer capable of producing the characteristic blue colour with the tincture of guaiacum.

Ozone passed through putrid blood destroys the putrescent odour, and restores coagulation.

The long continued action of Ozone on *albumen* gives rise to the appearance of "flocculi," resembling fibrine, and Mr. Smee, junior, believes these flocculi to be fibrine: but as they are not soluble in

nitric acid, this opinion must not be hastily accepted. M. Gorup-Besanez considers that the change undergone by albumen, when treated with Ozone, is in many respects similar to that caused by pepsin.

Upon *urea*, Ozone produces no greater oxidizing effect than does any other oxidizing agent.

Ozone produces a rapid action on *uric acid*, causing it to be soluble in water, which water, upon being slowly evaporated, yields yellow prismatic crystals strongly resembling allantoin, and, by a further evaporation of the water from which these crystals are obtained, a somewhat considerable quantity of urea is obtainable.

Allantoin, *alloxan*, and *creatin* are not acted upon at all by Ozone, and *leucin* but very slightly, if at all.

Ozone produces creatin from *creatinin*, and converts the solution from an alkaline into an acid one.

On *casein*, Ozone occasions a powerful effect, converting it at one stage of the experiment into a substance very like albumen; but a more continued action of the Ozone causes the formation of "flocculi," in every way resembling those produced by the action of Ozone on albumen.

Milk, through which a stream of Ozone has been passed for some time, will, if kept for a few days, lose all its casein, whilst the fatty particles of the milk will remain in an unaltered state for a very lengthened period.

Neither *fibrine* nor *bone gelatin* are at all acted upon by Ozone.

If Ozone be passed through *bile* containing all its natural constituents, it is very soon decolorized: but if the bile be freed from colouring matter, mucus, and fat, no effect is produced on it.

Inosite and *hippuric acid* do not appear to be at all acted upon by Ozone.

Whilst some chemists ascribe the various effects produced by Ozone on organic compounds to an oxidizing process, others, on the contrary, attribute them all to the production of mere decomposition.

CONCLUSION.

1.—The history of Ozone, through all the steps that have led to its discovery, is singularly interesting and beautiful: the discovery, throughout, has been conducted purely on the constructive or synthetic method, and, in all its early stages, has been the result of simple observation apart from theory.

Progress in regard to it has thus been made by long but even steps; each independent observation being confirmatory of what has passed before, and tending, ultimately, to the same end.

2.—There can be no escaping, at this moment, from the theory that Ozone is a modified condition of oxygen; indeed, *is* oxygen

plus force, which force is *probably* used in condensation, in other words, the power or capability of oxygen to combine with itself.

3.—The artificial production of Ozone, and the quantity that can be so produced, or, perhaps it is preferable to say, the quantity of oxygen that can be transformed into Ozone, is most clearly and distinctly defined; in fact, in the laboratory, the production of Ozone is an absolute verity as pure as the production of oxygen or chlorine.

For the production of Ozone in the laboratory, no method is so good as that accomplished by the aid of the "Induction Coil."

How Ozone is engendered in the air, if it be there, is not yet in any way definitely understood.

4.—The ordinary tests for Ozone are imperfect, not so much because they will not always prove the presence of Ozone, but because they prove too much; that is to say, the presence of other bodies also common to the atmosphere.

5.—In its action on the body, the effects of Ozone seem to be confined to the respiratory passages and structures, indeed, it is purely local in its action, closely resembling diluted chlorine and diluted bromine in vapour, the symptoms induced, varying in intensity, may be catarrhal, bronchial, or pneumonic; nor is there any evidence of any other class of diseases from Ozone.

6.—Ozone administered so as to produce death, promotes conditions clearly indicative, as I have already stated, of local action confined to the pulmonary system: the conditions are those of suppressed secretion, vascular congestion, and exudation.

7.—The relation of Ozone to disease, if there be such a relationship, is confined to disease local in its character, and to one part of the system—the respiratory: beyond this, there is no logical argument whatever, and even this must be accepted as problematical, until other potent influences are estimated as causes, on the principle of exclusion.

8.—On dead organic matter, Ozone exerts a powerfully destructive action, resembling, in this way, chlorine, iodine, and especially bromine.

9.—Ozone is a disinfectant and deodorizer, belonging to those bodies which disinfect and deodorize by resolving and decomposing into primitive and innocuous forms, competing, in this respect, with substances already named—chlorine, bromine, and iodine: it possesses these qualities in a less degree than chlorine and bromine, and is, in many cases, not so applicable as iodine. For a full and interesting account of the disinfecting and deodorizing capabilities of Ozone, as well as for a fair comparison between it and chlorine, bromine, and iodine, I would refer you to the opinions of the late Dr. Barker, expressed in the British Medical Association, Hastings Prize Essay, for 1865: the whole subject of disinfection and deodorization is there so admirably and exhaustively discussed as

to leave, it seems to me, nothing more to be said or desired in reference to this particular action of Ozone.

10.—As a preventive of disease, Ozone can only act by destroying organic animal poisons, in which respect it may again be compared with the substances I have more than once named.

11.—Lastly, as a remedy: in the form of Ozonized oil, of Ozonized ether, and Ozonized water, it once more ranks with a similar combination of remedies containing chlorine, bromine, and especially iodine. Whether, in any respect, it may prove to have greater advantages than the last named trusty and ready agent, can only be conclusively arrived at by determining whether it will do what iodine will *not* do, and this can only be decisively made out by applying to it the touchstone of inductive philosophy—a rigid exclusion of all that is ineffective.

It will be seen that throughout I have held to the positive, as far as it is known, of the effects of Ozone. Much has been written on the negative side, to prove, *i.e.*, what occurs when Ozone is absent from the air. I have studied these arguments, but do not consider they enter into my present work, in which I have had before me a distinct unity of intention.

And now, Mr. President, I must bring this imperfect report to an end, and, in so doing, I think it will be impossible for me to do better than by taking, for its final part, a few lines from the preface of a much more ambitious and far more deserving work. The lines are these :—

" If this publication be only a more solemn funeral of my
" remains, I wish it may be known that I die in charity and in my
" senses, without any murmurs against the justice of this age, or
" any mad appeals to posterity. I declare I shall think the world
" in the right, and quietly submit to every truth which time shall
" discover to the prejudice of these writings; not so much as
" wishing so irrational a thing as that any body should be
" deceived merely for my credit."

REPORT ON DISINFECTANTS.

By William Procter, M.D., F.C.S.

Volatile organic matter and its effluvia evolved during decomposition, more especially if derived from the animal kingdom, are amongst the most fertile causes of disease. This statement is so generally admitted, that it seems needless to detail the accumulated proofs tending to show that cholera, and other diseases of less virulency, as typhoid fever, etc., have been directly traced to deteriorations of this kind, introduced into the system either from the atmosphere by respiration, or from some other sources by way of the alimentary canal. If, then, this is correct as a fundamental fact, the enquiry arises of what do these effluvia consist, and in what manner do they operate in order to produce their prejudicial influence? Notwithstanding the most elaborate researches in physiological and chemical science which have been brought to bear on the subject, and although much has been done, it must be admitted that our knowledge in this matter is so far limited and deficient in that special exactness which could be desired.

Dead organic matter passes rapidly into a state of change or metamorphosis, the nature of the resulting products being dependent upon numerous collateral circumstances. Amongst these changes, putrefaction is specially distinguished by the fetid character of the products, resulting from decomposition. The more complex the composition of organic bodies, the more unstable is their equilibrium, the more readily do their constituents pass into a state of change, and the more offensive are the emanations; even the final products of putrefaction, particularly when air is excluded or imperfectly supplied, are highly odorous and deleterious, comprising, as they do, hydro-sulphuric acid, sulphide of ammonium, phosphides, ammoniacal and other like compounds, with volatile organic matter. It would not appear that the ultimate products of putrefactive decomposition directly generate zymotic disease, although they may produce their own peculiar effects; but that indirectly they promote its invasion by the exclusion of fresh air, form a nidus for the nourishment of morbific matter, and by depressing the powers of the system, render the body more predisposed and less resisting, and cause a more rapid spread of several specific diseases. It is rather to some intermediate product of the putre-

factive process, and especially to the organic effluvia still in a state of change, that the power of producing zymosis is to be referred. The evolved gases and vapours may either destroy life by acute poisoning, with symptoms well recognised; or produce a certain class of endemic diseases, not under ordinary conditions transmissible. Dr. H. Barker has submitted this question to the test of experiment, by conducting the air of a cesspool, containing carbonic acid and sulphides of hydrogen and ammonium, into a box in which animals were confined. The symptoms produced resembled the milder forms of continued fever, common to the dirty and ill-ventilated houses of the lower classes of the community, and Dr. Barker attributed the results, not to the organic, but gaseous matter. The effects of these gases were then separately tried on animals, and he found that of hydro-sulphuric acid 0·5 per thousand may produce serious symptoms, 2·06 per thousand may be fatal, and 4·29 per thousand kill rapidly, but not with the symptoms or pathological conditions of cesspool fever; while, on the other hand, a volatile alkaline body, as sulphide of ammonium, persistently administered produced both the symptoms and pathology of fever.

Dr. Dundas Thompson was one of the first to recognise the importance of organic matter as a constituent of the air of towns, and to enunciate the proposition that the gaseous products evolved during putrefaction are not the main sources of danger. The existence of a large amount of foreign matter in the air has been satisfactorily demonstrated. If pure distilled water is exposed in an open situation in a saucer, a fresh quantity being added daily to supply the loss by evaporation, and the sediment which is formed, submitted to careful microscopic examination, there will be found monads, amœba, and other low forms of life. Pasteur placed soluble gun cotton in a glass tube, and by means of an aspirator, caused a current of air to pass through it for several hours. The cotton being dissolved, the residue was found on examination invariably to contain organic growths, and he proved that the air of inhabited places contains a greater relative number of germs than the air of uninhabited localities. A glass globe filled with ice was suspended from the crown of a sewer, and by the cold thus produced, the aqueous vapour, with organic matter of the atmosphere, was condensed. In this manner, Dr. Letheby obtained from an ounce and a half to two ounces of liquid, which had a turbid appearance and disagreeable odour, an alkaline reaction, and contained flocculi of organic matter, which under the microscope was found to be composed of organic debris, with myriads of vibriones and monads; there were likewise present the higher forms of infusoria, with vegetable ovules and filaments of confervæ and fungi. Duchatelet with like experiments obtained similar results. It admits of ready demonstration, that air kept for a length of time in contact with putrescent

matter becomes loaded with oxidising material, and that such impure air facilitates the decomposition of meat, and rapidly changes milk. It is an eminently suggestive fact, that although the air contains the germs which are needed for the processes of fermentation and putrefaction, yet they are not everywhere present in all forms and in equal numbers. In the lower strata of the atmosphere they are numerous, fewer as we rise higher, and at the greatest elevations they are almost absent. Air taken on Mont Blanc was almost free from germs, as the height diminished the quantity increased, and reached its maximum in densely populated towns.

These conditions are universal, but in certain localities specific germs are detectible. In the wards of St. Louis, M. Chalvet collected putrescent organic matter from the walls, and when watery vapour near a suppurating surface was condensed, it was found to be strongly charged with irregular corpuscules resembling dried pus. Eiselt of Prague found small cells, like pus cells, in the air of a ward in which epidemic ophthalmia was raging, and to this dissemination of matter he very reasonably attributed the spread of the disease. There can be no doubt that this is one method by which these diseases, and others like them, are transmitted, although it is not at present in our power to recognise and demonstrate the special germs. Hence all circumstances which facilitate diffusion, such as a humid atmosphere, increase the spread of hospital gangrene, erysipelas, etc., whilst free ventilation checks it by dilution and removal of the poison.

These facts afford just reasons for concluding that the air is a medium through which diseases may be transmitted, either by specific germs or by the presence of organic matter undergoing putrefactive changes. Dr. Angus Smith has estimated that respired air contains 3·0 per thousand of animal matter, in the form of a putrescible, albumenized substance; and it is in impurities of this nature that evidence of zymotic action is to be found, whatever may be its origin. With regard to special poisons, their distinct existence has in many cases been demonstrated, and Dr. B. W. Richardson has separated the poison of pyemia. "It may," he says, "be evaporated to the form of a syrup or extract. It produces when dried a substance closely resembling the snake poison, it admits of being pulverized, and when it is introduced into the wound of a healthy animal, produces precisely the same symptoms as those of the patient from which the poison was taken." The experiments of Panum on putrid infection are of high interest, but he will not attempt to decide whether the putrid poison acts directly on the nervous system, or as a ferment on the blood.

In every instance of communicated morbid condition, there must be a material cause concerned in its propagation, however subtle may be its nature, however obscure its mode of transmission.

This material is in every probability made up of solid non-volatile organic particles. It may then be looked upon as a germinal cell, of which vaccine lymph may be taken as the type, of peculiar organization, capable of being transmitted from one locality to another, of preserving its vitality, for a time at least, outside the organism, and of reproducing itself within. There are two modes in which poisons may act on the system. Firstly, by growth, when a virus being introduced into the system it multiplies rapidly, and seems to be able to transform healthy matter into matter of its own kind, and to turn the albuminous portion of the blood or secretions into substances like itself, as in small pox. Secondly, by catalysis, a minute quantity of the virus being sufficient to set up the septic changes in the blood to which it has gained access. The history of the cases of deaths from such conditions, as the bites of venomous reptiles, shows that the essential action has been to set up putrefactive changes in the animal fluids. Fermentation possesses among organic principles, a much greater importance than is generally supposed; there exists between the processes of fermentation and several organic metamorphoses which take place in certain maladies a considerable analogy. An albumenized matter, in a certain state, acts as a ferment, either by contact or catalysis, and particular substances proceed from that action. The poison cell grows at the expense of the albumen or other element of the blood, and, may throw out matter as an excretion, so as to cause the special symptoms of the disease. Carefully conducted experiments by numerous observers have shown that, chemically and physically, the composition of the blood during disease undergoes alterations and variations.

Diseases may be artificially set up, closely resembling those which occur naturally, by introducing into the circulating fluids substances capable of operating as ferments or as catalysic agents. Abscesses have been produced by injecting pus into the veins of dogs, septic affections by the injection of putrid purulent matter into the veins, diseases with all the characteristics of typhoid fever by the introduction of putrid blood into the circulation, and contagious diseases such as glanders by the injection of glandered humours. Purkinje says that the blood in cholera contains urea, and an extractive substance by which the urea is rapidly converted into carbonate of ammonia.

But there is another element in the question which has to be considered: it has been shown that countless germs of vegetables and infusoria exist in the air, ready to develop themselves whenever they find an appropriate nidus. This condition of the atmosphere derives importance from the possibility of its being concerned in the production of zymotic disease. That these low forms of organic life may seriously affect the blood of the higher order of animals, is proved by the researches of Davaine, who has furnished the first well established example of blood disease due to the presence of

inferior beings. The bacterium, a low form of organization, of obscure nature, but which is probably one of the confervoid algæ, was discovered in the blood of sheep suffering from splenic apoplexy, and was considered by this observer to be the cause of the disease. Dr. Salisbury of Ohio states that the prevalence of measles in the Federal army arose from a fungoid penicillium. He was led to this examination by observing that a large number of men arose one morning, with symptoms of measles, after sleeping on straw which was mouldy and had a peculiar odour. He inoculated himself, his wife, and forty other persons with the fungus, producing in all of them a disease like measles in from twenty-four to ninety-six hours. It is nevertheless proper to remark, that Dr. Woodward, of the U. S. army, has repeated the experiments of Dr. Salisbury, and does not confirm them. Dr. Schmidt, Dr. Salisbury, and Mr. Massey, have lately brought forward very strong evidence, tending to show that malarial fever, if not caused by, is at least intimately associated with, certain cryptogamic growths which they describe.

The atmosphere then abounds with substances capable of generating disease, and although we are at present unable to define the sphere of operation of each variety, or to settle various questions connected with their precise etiology, the general fact is indisputable. If this be so, two important questions arise, is it within our power to remove these causes of disease? or are there any means of preventing or diminishing the injury which may arise from the morbid conditions produced by their presence?

Chemistry has supplied such methods, in a class of substances called in their widest sense Disinfectants, and which act chemically on infectious matter by one of two methods. (1) By a process of oxidation which rapidly effects its decay; or (2) by preventing any change of its original composition. Here then are two great divisions, differing essentially in their *modus operandi*. (1) *Disinfectants*, which hasten the decomposition of the organic compound into its simplest form, having as the result harmless products. (2) *Antiseptics* or *Colytics*, which place the organic matter in such a condition that any change is restrained or prevented. To the preceding divisions another may be practically added, and to which the term *Fixative* has been applied, such as Burnett's fluid, etc. One or more of the constituents of these agents, by entering into combination with the offensive volatile products, *fixes* them, and prevents the pollution of the air by their escape.

Substances capable of removing bad smells, of removing in fact the noxious gases and vapours emitted from putrefying organic matter, and called *deodorisers*, are often considered synonymous with disinfectants. But this is an error, for there is no evidence to show that infectious disease is *of necessity* associated with odorous matter. Bad smells may and do become injurious if much

concentrated, by the introduction of poisonous gases and vapours, and the consequent exclusion of a due supply of pure air. In our houses for example, if the drains and other sources of impurity are allowed to give off miasmata, they most certainly become causes of disease; but in the open air, where they are largely diluted, the injury of these emanations to man is materially diminished. A bad odour is an indication that ventilation at least has been neglected, and may indeed afford valuable index to infection; but if this is removed by a deodoriser, then the safeguard afforded by the smell becomes nugatory, the unpleasant odour is absent, but the true poison continues and accumulates.

The term Disinfectant is in this paper limited to oxidizing disinfectants. Oxygen, on account of its wide chemical affinity, is the great purifier of nature, and the great natural disinfectant, carried on most energetically by the allotropic form, ozone. When we imitate this great oxidizing process, the action is complete, decided, and perfect, as exemplified in combustion, the action of air or chemical agency by means of oxidizing disinfectants, and it is to this chemical agency that attention has here to be especially directed. In comparing the modes of action of disinfectants and antiseptics this essential difference is seen to exist between them, that the former act primarily and mainly on the volatile products of decomposition, the latter act at an earlier stage by preventing that decomposition from taking place. Hence disinfectants changing by chemical action the offensive gases of decomposition, as a consequence have their energy destroyed and their power neutralised (unless they are in great excess) before they are able to exert any influence on the real source of mischief, the decaying organic matter.

Nitrous acid and chlorine act by oxidation; the former directly, the latter indirectly, furnishing oxygen. The former contains much loosely combined oxygen, which it readily gives up, being converted into binoxide of nitrogen; this, by fresh union with a further supply of oxygen, again becomes nitrous acid; so that this gas acts as a carrier of oxygen to atmospheric organic matter, and destroys it rapidly. Dr. Macbride, in the middle of the last century, wrote a treatise on disinfectants, giving the results of numerous experiments, and showing especially the great power which acids possess in this direction. Dr. Carmichael Smith afterwards, in the hospital at Winchester, used nitrous acid with eminent success; and in the fleet at Sheerness, in 1785, its employment was mainly the cause of the removal of a pestilential fever; for this application, Dr. Smith received from the Government a grant of £5,000. There can be no doubt that the action of nitrous acid in the destruction of organic matter is very perfect; but there are several reasons which prevent the adoption of it for disinfecting purposes in inhabited dwellings. The vapour in small quantities is more or less injurious to the respiratory organs; and as Mr. Crookes has pointed out, being absorbed, it forms, with the

walls and ceilings of the rooms, a deliquescent compound which tends to keep up a condition of moisture, likely to assist the spread of infection. Still the evidence of the advantage which has attended the evolution of nitrous acid fumes in fever wards is irresistible, when measures are taken to distribute it equally and in proper quantity.

The disinfecting powers of *Chlorine* were noticed by Berthollet shortly after the discovery of that gas in 1774, but its employment for this purpose cannot be dated farther back than the commencement of the present century, when Guyton, Morveau, and Dupuytren pointed out its great value. Chlorine is an energetic destroyer of all organic substances prone to undergo decay, liberating the nitrogen in a gaseous state, breaking up into simpler forms ammoniacal, sulphuretted, phosphoretted, albuminous, and cyanogen compounds; but exercising its influence rather upon dead than living matter. An experiment of Mr. Crookes' well illustrates this statement. Cheese mites were put into water mixed with strongly-smelling cheese and sulphuretted hydrogen, and into this mixture a solution of chlorine was gradually dropped from a burrette. The odour was destroyed when ten divisions had been used, but thirty were needed before the mites were destroyed. The experiment was repeated without the sulphuretted hydrogen, and here, the chlorine having nothing to divert its energy, the mites were killed with a quarter less of the chlorine solution.

These properties place it high in the rank of deodorisers. In the state of gas chlorine has great powers of penetration, destroying everything which it attacks, but in killing it dies; for that which is left after its action is exhausted has no power of resisting further decomposition conferred upon it. When meat is boiled with hypochlorite of lime, with the addition of sulphuric acid to liberate chlorine, it becomes oxidized and passes away in harmless products. In this destructive power greater activity is displayed than by oxygen, arising, in all probability, from the liberation of that gas in a nascent condition. In cases of great impurity, and where rapid action is needed, chlorine is preferable to nitrous acid; but similar objections to its use exist in its effect on the respiratory organs and the production of a deliquescent salt with lime.

In ordinary cases of fumigation the senses seem to be the best test of the quantity to be used; if there is chlorine enough in the air to give a faint smell, it will be found in sufficient quantity to disinfect. There are many cases in which the so-called chloride of lime may with advantage be substituted for the gas; and when this is used the addition of a few drops of nitro-benzole will mask the unpleasant odour.

Iodine and *Bromine*, chemically allied to chlorine, have, like that substance, great powers of removing impurities from the atmosphere or liquids. Bromine is said by Chevallier to be

the more energetic of the two; but the expense, unpleasant odour, and extremely irritating nature of the vapour, are powerful practical arguments against any very general employment of this substance. A greater amount of attention has been paid to iodine. Dupoy in 1854 showed that it was a powerful arrester of putrefaction, and in 1855 proved that by its agency the poisonous property of curari was destroyed; and Richardson, Wynn Williams, and Nunn, have used this substance for the purposes of disinfection. In a moderately warm place, iodine readily diffuses spontaneously, or this diffusion may be accelerated by the application of heat. Dr. Richardson proposes to saturate a solution of peroxide of hydrogen with iodine, adding two and a half per cent. of sea salt, and to diffuse this mixture where required, in the form of spray, by means of his instrument. The vapour of iodine is condensed with facility, a circumstance which must retard or prevent extensive diffusion; hence the scope of utility is, on this account, probably more limited than that of chlorine, In every other respect it is equal, if not superior, to the latter gas.

The *Alkaline Manganates and Permanganates* in solution have been introduced by Mr. Condy, of Battersea, into the list of disinfectants. They contain a large quantity of oxygen, part of which they freely give up to organic matter, and destroy it rapidly by oxidation, or rather, as the inventor considers, by the action of ozone. In this respect they differ from chlorine and its allies, which act indirectly by the decomposition of water, while the manganates directly supply one fourth, and the permanganates three-eighths of the oxygen, they respectively contain; peroxide of manganese being in both cases precipitated, and the alkaline base remaining in solution as a carbonate. In this manner, the decomposition of organic material to the last and innocuous stage is rapidly effected, the odours of putrefaction are destroyed, and the active agents, productive of certain diseases, are consumed: and, moreover, they effect the decomposition of sulphuretted hydrogen, or sulphide of ammonium, when they are present. The principal application of these solutions is in the disinfection of liquid matter or water charged with organic material, and this, if of the most offensive character, is immediately deodorized by their addition; and in some cases, when filtration through animal charcoal has been ineffectual. These properties render Condy's fluid a most valuable test of animal or vegetable matter in air or water, indicated by the loss of the well marked pink colour naturally pertaining to them. Mr. Condy has likewise shown that these salts have the power of removing from water any contamination by lead, by oxidizing the protoxide of that metal, and precipitating it as the peroxide. Condy's fluid not being volatile acts mainly on fixed substances, and from its energetic action upon all organized substances, it destroys clothing, etc., having its

efficacy at once destroyed by neutralization; hence, in its concentrated form at least, its employment in this direction is limited. Nevertheless, for numerous purposes, especially when expense is not a chief consideration, Condy's fluid is of the highest value, such as the disinfection of alvine discharges, of ulcers or cancerous sores, the purification of tainted provisions, etc.; but, like all oxidizers, whilst it destroys that on which it acts, decay is not prevented from being again set up. Deleterious action on organic life is not a very marked feature of these agents, for it is certain that animalcules will live and be active in water retaining the pink colour given to it; therefore the purification by these means alone cannot be considered perfect, but should, to insure entire removal of organic life, be supplemented by boiling.

The *Antiseptics*, called also *Colytics*, are a class which have the power of preventing organic substances from undergoing chemical change, either by arresting catalysis or oxidation, or by preventing other methods of decomposition; but their precise *modus operandi* is obscure. They in fact disinfect by preservation, and in many cases seem to arrest change as freezing would do. They who advocate the organic character of fermentation and putrefaction, and the doctrine that the changes are essentially associated with minute organisms, believe that antiseptics prevent the metamorphosis, by unfitting the medium to nourish the ferments; whilst the supporters of the chemical theory, ascribe the arresting influence of these agents, to their action upon bodies in a state of change. Oxidizing disinfectants exercise their peculiar influence upon substances which are the results of changes already effected; but antiseptics prevent these results, by destroying decay and decomposition in substances which come under their influence, so that whilst a colytic preserves from putrefaction, it does not of necessity remove the odour of that matter which has previously undergone change. Substances which remove putrid matter are disinfectants; when by their influence chemical change is stayed, they are antiseptics; but when the odour is removed by a substance which at the same time prevents putrefaction or other decomposition, then the most thorough disinfection is attained. Heat and cold are natural antiseptics. Extreme cold prevents animal poisons from being diffused and oxidized, and restrains putrefaction. The greater number of animal substances may be indefinitely preserved at or below the freezing point, becoming subject to change with an elevation of temperature. Extreme heat destroys the chemical structure of morbid poisons and products, assists the action of oxygen, and, in some instances, may operate beneficially by producing expansion and consequent dilution of the noxious material. It is to Dr. Henry that we are indebted for a knowledge of the powerfully destructive influence exerted by dry heat upon specific poisons. Vaccine virus was deprived of the power of reproduction after exposure to a tempera-

ture of 140° for three hours: but this result did not follow when the heat was not above 120°; from this and other similar facts, he was led to suggest the adoption of a method for the disinfection of clothing and the like, which consisted in exposing them to a temperature of from 212° to 214°, and experience has fully corroborated the perfect success of the plan when carried out with proper care and continued for a sufficiently long time.

The fumes of sulphur are amongst the most ancient disinfectants, held sacred in past ages on account of their wonderful efficacy. According to Dr. Graham, "*Sulphurous Acid* is preferable to chlorine. No agent checks in so effectual a manner the first development of animal or vegetable life, all animal odours and emanations are effectually and immediately destroyed by it." By chemical action, the odour of sulphuretted hydrogen, phosphuretted hydrogen, ammonia, and other foul gases, is removed, and at the same time ammonia is fixed without loss of antiseptic power. The value of sulphurous acid is mainly due to the character possessed by it as a deoxidizer, and in this manner being constituted a powerful colytic and deodorizer, takes a high stand for disinfecting purposes; it is effectual, economical, of considerable permanence, and readily applied: the great objections presented, are the irrespirability and poisonous properties of the gas, coupled with a disagreeable odour, characters which render its employment, except in uninhabited places, impracticable.

The combinations of sulphurous acid, the *Sulphites*, the alkaline salts of which are soluble, have been particularly investigated by Dr. Polli: he finds them to possess all the properties of the acid, but with a more certain, uniform, and constant action. When animals, to which the sulphites had been administered, were killed, they were not prone to decomposition, and remained fresh for a very long time. The most putrid and offensive blood becomes comparatively innocent by the addition of sulphite of soda, but it does not become altogether inodorous, although all perhaps that is really noxious is removed; these salts go on acting until eventually sulphuretted hydrogen gas is liberated, a chemical change which takes place more particularly when a large amount of liquid is present. Dr. A. Smith and Mr. M'Dougal have combined the sulphites of lime and magnesia with carbolate of lime, producing a disinfectant as valuable as any hitherto fabricated, although I think that produced by Mr. Calvert has some advantage over it in antiseptic properties; this opinion being arrived at, by submitting meat, blood, urine, etc., severally to their influence under precisely similar conditions. One great advantage is the continuous action due to the liberation of the acids through the operation of the atmosphere. A great cause of disease and discomfort in towns and farms, particularly in stables, is the presence of water; these powders spread upon the floor assist in the removal of moisture, and, absorbing the

ammonia and phosphates, produce a valuable manure after they have exercised the other beneficial effects. When a solid is not required, the operation is equally good in solution. They are invaluable for mixture with fecal matter which cannot be immediately removed from any locality, and the inventors propose to disinfect sewage as well as sewers with them. By application to the sewage water, the impure liquids pass disinfected through their course, purifying the town and sewers at the same time.

The antiseptic value of empyreumatic substances has been known for a very long period, and their use in this direction is very general; amongst them, *Carbolic Acid* is now the most important. The properties now under consideration were first observed in coal tar creosote, composed of cresylic and carbolic or phenic acids, and which, boiling at different temperatures, are capable of separation by fractional distillation. They are both antiseptic, but the latter is in more general use. Twenty years ago, Laurent pointed out a method of extracting carbolic acid from coal tar, but it was by no means pure; at this time, Dr. Crace Calvert has succeeded in producing a crystalline acid, fusible at 41°, boiling at 182°, and soluble in twenty parts of water; it is also free from disagreeable odour and tarry flavour. This form is more especially adapted to medicinal uses, the impure liquid acid being the state in which it is used for disinfecting purposes. A saturated solution of carbolic acid destroys plants rapidly, fishes and leeches die in it, and on exposure to the air their bodies dry up without putrefaction, it makes turbid weak (but not strong) solutions of gelatin, and coagulates albumen. In such a liquid, flesh becomes hard and shrivelled, remaining in this condition for years without decomposition; whilst, in very small quantities, both animal and vegetable ferments, as well as the lower forms of life, are destroyed by its influence. But for these purposes solutions of a certain strength are needed, if below that standard the fermentative or putrefactive change is not arrested; thus, in relation to the saccharine fermentation, one per cent. retards it, one in 50 stays it, but one in 1,000 has no effect on the metamorphosis. In the same manner, there is a limit to the preservative power of the tar acids: flesh putrefies in a liquid containing one part carbolic acid in 500 of water, but is completely preserved in one part to 60 of water. From these facts then it follows, that in order to obtain the full effect, a sufficient quantity must be employed; and it would appear that such graduated amounts may be applied, as either to arrest putrefaction or the lower organic phenomena, or to attack the higher vegetable and animal life. Carbolic acid appears to arrest the motion which takes place in decay, and is therefore a true antiseptic. The precise manner in which it acts is as yet an unsettled question; by some the power to stay decomposition has been considered identical with the property of coagulating albumen. A solution however of such a

strength that albumen is not coagulated by it, in some cases prevents putrefaction; and Mr. Crookes has shown that a liquid, from which all the albumen was not precipitated by carbolic acid, underwent no change.

By testing the action on various ferments, Lemaire has obtained singular and interesting results. These experiments go to show that carbolic acid, of sufficient strength to destroy the vinous, acetous, and lactic fermentations, does not affect the conversion of starch into sugar by diastase, nor the formation of oil of bitter almonds. From this it would appear that the acid has no influence upon purely chemical ferments, but arrests those fermentations which depend upon the development of organic life; while, on the other hand, Voelcker says that the power of rennet to coagulate milk is only materially altered when brought into contact with strong carbolic acid. The powerful action on cell life is further illustrated by the fact, that the power of vaccine virus to exert its action is prevented by admixture with this substance.

In a state of vapour it exerts a great preservative power as well as destructive influence on many forms of animal and vegetable life. Used out of doors in yards or other like places, by being poured on the ground vapour is given off, the result being that both the air and soil are disinfected at the same time. Exerting its influence, as does carbolic acid, on substances which may from changes taking place in them become sources of disease, it is especially adapted for employment in sewers, cesspools, etc., and has been used with the most marked success at Exeter, London, and other places, and the British Government have ordered it to be used in their departments, to the exclusion of all other disinfectants. But valuable as it is, there are cases where moisture has to be avoided; and the odour is objectionable, which may preclude its use; but the latter difficulty is removed, when solutions of the pure crystalline acids are used. All practical men are agreed, in relation at least to the common liquid carbolic acid, that it is a great error to employ the antiseptic in a concentrated state. Water dissolves about four per cent. of the acid, and this liquid will in ordinary cases be found sufficiently strong, and to work efficiently. It is an unfortunate circumstance that a fictitious mixture of coal oils, containing but little of the real acid, is extensively sold, leading to disappointment in the beneficial effects. Mr. Crookes has put into our possession a simple and approximative test of the value of a given specimen. Commercial carbolic acid is soluble in twice its bulk of a solution of caustic soda, while oil of tar is nearly insoluble. The test is thus carried out:—Dissolve one part of caustic soda in ten parts of warm water, and shake it up with five parts of the carbolic acid to be examined, when the amount of oily residue will show the impurity.

Under the term *Fixative Disinfectants* are comprised the metallic salts of iron, zinc, lead, etc., variously combined with sulphuric

acid, nitric acid, chlorine, etc. Their operation is mainly limited to the offensive gases of putrefaction, and that according to the salt employed: for example, Burnett's fluid operates principally upon sulphide of ammonium, and but little upon hydro-sulphuric acid; but the perchloride of iron exercises a decomposing influence upon both. Whilst then the fixative class destroy the ammoniacal and sulphur compounds, a large number of other compounds, offensive and injurious, are confined by neither constituent of the metallic salt; moreover the matter acted on is not endowed with an indisposition to undergo further chemical change, and the gases which they retain being readily separated, the renewal of the agent is frequently demanded. Their use seems to be especially indicated when large masses of putrefying matter have to be deodorized for a limited time and at a small cost. These considerations led to the employment of this class of agents by Drs. Hoffman and Frankland for certain operations in the metropolis, when the deodorization was required to last long enough to allow the descent of the matter in an inoffensive condition to the estuary of the Thames.

I have thus far endeavoured to describe, in a brief manner, the general characters and properties of the disinfectants in ordinary use, and to avoid trespassing too long on your time, I have omitted a detail of experiments which have been made in support of the statements; for a similar reason the consideration of charcoal as a sanitary agent has been passed over. In the selection of a disinfecting agent which may be demanded to fulfil a special object, we shall have to be guided by a consideration of these properties, which, besides pointing out the applicability of a given substance to effect the object in view under the particular circumstances, will prevent the medical man from falling into that not unfrequent error of adopting two or more disinfectants from the different classes whose operation might be antagonistic or naturally destructive. The attempt to obtain a disinfectant, admitting of general application, and capable of fulfilling every indication, must be attended with disappointment. When the use of these materials is called for, either directly or indirectly, decomposition of some kind has to be dealt with. As in fermentation the products vary with the nature of the ferment and the fermentable material, coupled with external conditions in relation to air, water, and temperature;—thus, if sugar be taken as the type, it may undergo the vinous, acetous, lactic or other fermentation, according to the conditions to which it may be submitted;—so with other substances different results will arise according to circumstances, and different products will have to be dealt with. Disinfection therefore is not a simple process, but depends upon varying chemical and physiological actions.

In disinfecting the chambers of the sick, the kinds of impurity which have to be contended against are manifold and varied;

in addition to air vitiated by respiration, there are exhalations, general or special, from the body of the patient and the effluvia of excretions; and the operation will have to be exercised, with some exceptions, rather upon the products diffused through the air, than upon the sources themselves. Here then a disinfectant possessed of diffusive powers, combined with rapid action, will be selected. The oxidizing class fulfil these indications, and chlorine, used with care and caution, and liberated in graduated quantities from small bottles of the gas, is of the highest value, but may frequently be replaced advantageously by the hypo-chlorite of lime. Setting aside the objection to which, as a dense, readily condensable vapour, it is open, iodine is in many respects superior to chlorine; its action is more powerful, it is manageable, and it can be volatilized to any required amount by regulation of the temperature to which it is submitted. In the majority of cases no heat beyond that of the apartment is needed, the simple exposure of iodine in a plate or saucer will diffuse a sufficient quantity of vapour to keep the air pure and inodorous. Besides chlorine and iodine, ozone may for this purpose be advantageously employed. A ready method of making ozone is, after scraping sticks of phosphorus, to place them in a bell jar, and half cover them with water. The air in the bell jar is speedily ozonized, and from this source the chamber may be readily supplied with the active agent; but when ozone is required in any quantity, the apparatus invented by Siemens is the best, in which the ozonized air is produced by the aid of sparks from an induction coil. Mr. Condy proposes to liberate ozone in an apartment by the addition of sulphuric acid to his permanganate solution, and likewise to disinfect by dipping cloths in his solutions, and hanging them up in the sick room; but the violent action of permanganates on all organized material must speedily neutralize their influence. It has also been suggested to distribute the fluid through the air, mixed with the vapour of water; a plan of which I have but limited experience, and which is apparently open to the objections of an undue diffusion of meisture through the chamber.

In the disinfection of places which are uninhabited, as empty houses, stables, etc., the characters of sulphurous acid render it especially applicable for such purposes, and preferable to nitrous fumes. The rooms, etc., should be closed, a quantity of sulphur burnt in different places, and the process followed by whitening with *quicklime* wash. The removal of infection or contagious matter from bedding or wearing apparel, may be carried out most simply and effectually by the action of dry heat. This should be done in chambers of masonry or metal, the articles being arranged on shelves, or suspended, so that when heat is applied their entire surface is submitted to its influence. From experiments made on various materials employed for clothing, etc., it appears that they may be exposed to a dry heat of 250° without

injury to their texture, while a temperature of 212° is sufficient for the destruction of infectious matter. It is important that the articles to be disinfected should be exposed to the heat for a sufficiently long period. If circumstances render it desirable to make use of a gaseous disinfectant, sulphurous acid is the best, its action being continued for several hours with subsequent exposure to light and air.

When liquid or a mixture of solid and liquid matter has to be dealt with, those substances which may become noxious having to be restrained, reliance will be placed on the antiseptic and fixative class. For the disinfection of urinals, drains, middens or sewers, carbolic acid, Calvert's or M'Dougal's powders take precedence of others, producing a substance of high agricultural value by the retention of the ammonia and phosphates; although on a small scale, and as temporary deodorizers, Condy's fluids and the chlorine compounds are of the greatest service and act rapidly. To remove the odour, etc., of alvine discharges, carbolic acid and its preparations are applicable; but on account of the odour, and for other reasons, the sulphate of iron, nitrate of lead, (which I prefer,) chloride of zinc, or perchloride of iron, may with advantage be substituted. It must not be forgotten that the principal advantages of this class lie in their power of retarding the decomposition of organic matter, and of being inodorous; but their efficacy in the destruction of specific emanation is by no means proved. They are eminently of service in removing unpleasant smells from the rooms of the dead, when placed about the corpse.

From this summary it would appear that there are, in general, two classes of cases in which the use of disinfectants may be needed.

(1) When the air is impure from causes not under our control, as in epidemic or zymotic disease.

(2) When the air is impure by causes under our control, such as want of cleanliness, local sources of impurity, etc.

In the first case, gaseous disinfectants are had recourse to, for the purpose of destroying infection by their diffusion; while in the second, reliance is placed upon liquid or solid disinfectants, that infective matter may be prevented from passing into the air. Whilst it is true that disinfectants, properly and judiciously applied, admit of extensive uses in sanitary medicine, and are valuable adjuncts in removing the sources and preventing the extension of diseases, their advantages must not nevertheless be overrated. In defining their limits, it must be recollected that these agents are not substitutes for ventilation and cleanliness; in fact, if no preventive methods are used, they may be worse than useless, by leading to a confidence which the experience of their employment does not justify. The atmosphere has chemical and physical properties, which enable a state of purity to be retained,

by the oxidation, diffusion, etc., of noxious matter introduced into it. Being always endowed with these powers, the importance of a continuous and abundant supply to the chamber of the sick is evident. The oxidation destroys and the diffusion dilutes noxious matter, it may be to the point of innocence, while the mechanical movements aid in the entire removal of that which is injurious. But whilst these natural operations may receive important assistance by artificial disinfectants, the latter can only be looked upon as adjuncts to ventilation; if that is neglected, their security alone against infection and disease is not to be relied upon.

ON METEOROLOGY IN RELATION TO EPIDEMIC AND SPORADIC CHOLERA, AND OBSERVATIONS ON OSMOSE, WITH PRACTICAL DEDUCTIONS.

By T. Moffatt, M.D., F.R.A.S., F.G.S.

In the year 1848 I observed that cases of diarrhœa occurred with changes of weather and with the reappearance and increase in the quantity of atmospheric ozone; and in 1849 it appeared to me that sporadic cases of cholera were accompanied by atmospheric changes, and that epidemic cholera prevailed when ozone was at a minimum, and disappeared when it increased in quantity.

From results deduced from the meteorological observations of two years, viz.: 1850 and 1851, I observed that ozone periods commenced, or ozone increased in quantity, with decreasing readings of the barometer and the setting in of the equatorial or south current of the atmosphere, and that they terminated, or ozone decreased in quantity, with increasing readings of the barometer and the setting in of the polar or north current.

From results of observations taken during the cholera epidemic at Newcastle, in 1853, I had no doubt that there was an intimate connection between the prevalence of the disease and the absence of atmospheric ozone, and the decline of the disease and the setting in of the ozoniferous current of the air.

In the following table I have given the mean reading of the barometer and thermometer, the mean daily quantity of ozone, the degree of humidity, and the direction of the wind, for August, 1853, before cholera appeared at Newcastle, while it was increasing, and while it was declining.

Table I.

Mean Weekly Reading of Barometer.			Mean Weekly Temperature.		Mean Degree of Humidity.		Daily Mean of Ozone			Mean Direction of the Wind.			
										Cholera Increasing.		Cholera Declining	
No Cholera.	Cholera Increasing.	Cholera Declining.	Cholera Increasing.	Cholera Declining.	Cholera Increasing.	Cholera Declining.	No Cholera.	Cholera Increasing.	Cholera Declining.	N.	S.	N.	S.
29·398	29·812	29·609	55·4	54·8	82	82	4·7	1·3	5·6	9	6	3	11

By this table it appears that there is a marked difference in the meteorology of cholera and no-cholera periods. The first case of cholera, in 1853, at Newcastle occurred on the 31st of August, and from that date until the 19th of September the barometer readings were high, the wind was more frequently in north than in south points of the compass, and the quantity of ozone was much below the mean. On the latter date it was 1·0, and the number of deaths from cholera was 108. On the 20th of September the barometer readings were decreasing, and the wind had set in from south points; ozone increased in quantity, and continued to increase until the 28th, and on that day the number of deaths was 18 only, and the mean daily quantity of ozone 5.

Mr. Glaisher, in his valuable "Report on the Meteorology of London during the Epidemic of Cholera in 1854," observes that, during the height of the epidemic, the reading of the barometer was remarkably high; a total absence of rain, and a stillness of the air, almost amounting to a calm, accompanied the progress of the disease, and there was a deficiency of electricity and a total absence of ozone. The first decline of the disease was marked by a decrease in the readings of the barometer, and in the temperature of the air and water. The calm was succeeded by a strong wind from S.W., which soon dissipated the former stagnant and poisonous atmosphere. "The barometer," he continues, "again increased, the air became again stagnant, and the decline of the disease was considerably checked." He also states that cholera was most fatal in low situations; and my own observations, with regard to height, show that the greatest number of deaths from ordinary causes occur, with minimum of ozone, in the lower stratum of the atmosphere, while the least number takes place in the higher stratum, with maximum of ozone.

In 1854 and 1855 a series of experiments was instituted at Strasbourg, during the epidemic at that place, and the conclusion arrived at was, that there was an intimate relation between the development of the epidemic and the diminution or disappearance of ozone; and in the "Report on the Registration of Ozone in the Bombay Presidency, for the Year 1863 and 1864," by Dr. Cook, it is stated that there is a decided connection between the absence or marked decrease in the quantity of ozone and the prevalence of cholera.

There is also a marked decrease in the quantity of ozone during the prevalence of choleraic diarrhœa in the autumn months. The results in the following table are from the observations of ten years.

TABLE II.

MONTHS.	January, February, March.	April, May, June.	July, Aug., September, October.	November, December.
Mean number of Cases Diarrhœa	8·8	8·5	13·5	7·3
Mean of Ozone.....	2·0	2·0	1·0	2·0

The conditions of a choleraic atmosphere are high pressure and temperature, a calm or gentle motion of the air from points of the compass between W. and E. by way of N., minimum quantity or absence of ozone, absence of clouds of the cirrous class, and a peculiar haziness called by Mr. Glaisher "Blue Mist." These conditions terminate with decreasing readings of the barometer, varying temperature, and direction of the wind from points between E. and W. by way of S., a reappearance of ozone or an increase in its quantity, clouds of the cirrous class, and sometimes with rain and a thunder storm. These conditions were well marked during the epidemic of last year (1866), but the barometer readings were neither so high nor so steady as with former epidemics. They were, however, above 30in. on three days in the week ending July 14th, and on five days in October.

In the months of July, August, and September of last year (1866) cholera was diffused over nearly every county in England and Wales, and it destroyed 10,365 persons; and in the quarter ending December 31st the deaths from it were 2,465. During the former quarter atmospheric ozone was below the mean in Great Britain and Ireland and in the Channel Islands, and during the latter quarter it increased much in quantity. Taking the mean daily quantity of ozone, for three years, for the quarter ending September, in Great Britain and Ireland and the Channel Islands, we find that it is 4·5, while for the same quarter of last year (1866) it was 3·0 only; so it would appear that the diminution of ozone was general over the country while the epidemic lasted.

TABLE III.—*Showing the Meteorological Conditions of the Cholera Period of* 1866, *from June to December, and the Number of Deaths in London Weekly.*

Week Ending	Mean Weekly Reading of the Barometer.	Mean Temperature.	Mean of Ozone.	Mean Direction of Wind. N.	Mean Direction of Wind. S.	Number of Deaths from Cholera and Diarrhœa.	Increase of Deaths in London.	Decrease of Deaths.	REMARKS.	Mean Daily Amount of Ozone.—Cumberland, Ireland, and Flintshire.	Class of Cloud, Rain, Haze, &c.	Week Ending
June 9	29·739	60·2	2·0	0	14	..	..	..	.. .. ..		Rain on 5 days, thunder 2, haze 1; class of cloud, cumulo-[stratus.	9 June
16	29·557	58·3	3·0	1	13	..	..	..	.. .. ..	4·0	Cirro-stratus, cumulo-stratus, no haze.	16
23	29·537	49·0	2·0	1	12	..	..	..	.. .. ..		Cumulo-stratus, no haze.	23
30	29·837	69·4	1·0	7	7	73	73	..	.. .. ..	1·7	Cumulo-stratus and cirro-stratus, haze 6 days.	30
July 7	29·267	57·1	1·3	5	9	116	43	..	Cholera appeared at Liverpool.		Cumulo-stratus, haze on 2 days.	7 July
14	29·960	72·1	1·0	7	7	182	66	..			Cumulus, cumulo-stratus, haze on 3 days.	14
21	29·901	64·6	1·1	8	5	567	385	..	.. .. ..	2·5	Cumulus, haze on 4 days.	21
28	29·810	62·2	0·2	12	2	1,254	686	..	.. .. ..		Cumulus, haze on 6 days.	28
Aug. 4	29·411	58·4	2·5	6	8	1,407	154	..	No increase in number of deaths at Liverpool.		*Cirri*, cirro-stratus, cumulo-stratus, thunder 2, and rain.	4 Aug.
11	29·379	57·1	2·0	5	8	1,045	..	362			*Cirri*, cirro-stratus, and cumulo-stratus.	11
18	29·599	58·3	2·3	9	5	649	..	396	Decrease of 11 deaths at Liverp.	3·5	Overcast, haze 5 days.	18
25	29·706	63·0	0·4	4	8	394	..	255	.. .. ..		Rain on 4 days, haze 1, thunder and lightning.	25
Sept. 1	29·894	60·0	2·0	5	9	326	..	68	Decrease of 80 deaths at Liverp.		*Cirri*, rain 6 days, haze on 1 day.	1 Sept.
8	29·675	57·3	1·3	5	9	289	..	37	.. .. ..		Rain 6 days, haze on 2 days.	8
15	29·362	55·8	2·2	0	14	292	3	..	Increase of 37 deaths at Liverp.		*Cirri* 2 days, rain 7, haze 2.	15
22	29·260	57·3	1·8	4	10	248	..	44		1·5	*Cirri* 3 days, rain 4, haze 3, fog 2.	22
29	29·411	46·8	0·6	2	9	244	..	4	.. .. ..		Rain 2 days, fog 1, haze 1.	29
Oct. 6	29·981	59·2	0·0	10	4	251	7	..	Cholera at North Shields, Sunderland, Chester, Flintshire, Devonshire, and Dublin.		Fog 1, haze 7 days.	6 Oct.
13	29·973	51·5	0·4	5	6	254	3	..			*Cirri* 2 days, rain 2, haze 4, fog 1.	13
20	29·797	48·4	0·2	1	12	199	..	55			*Cirri* 4, rain 3, solar halo 2 days, haze 2.	20
27	29·674	52·7	0·8	1	13	144	..	55	.. .. ..		*Cirri* 6, solar halo 1, rain 4, aurora 2.	27
Nov. 3	29·703	49·9	1·2	3	11	101	..	43	.. .. ..	3·5	*Cirri* 4, solar halo 1, rain 4, hail 1.	3 Nov.
10	29·648	49·1	1·3	1	13	100	..	1	.. .. ..		*Cirri*, solar and lunar halos, rain 5, hail 2.	10
17	29·537	45·5	2·3	7	7	54	..	46	.. .. ..		Rain 4 days, sleet 1, haze 1, frost.	17
24	29·747	41·1	2·7	6	8	34	..	20	.. .. ..		.. ..	24
Dec. 1	..	..	..	..	..	18	..	16	.. . .	..	.. ..	1 Dec.

From these observations we have the following results.

TABLE IV.

Mean Reading of the Barometer.			Mean Temperature.		Daily Mean of Ozone.			Daily Mean of Ozone, Ireland, Cumberland, and Flintsh.			Mean Directions of the Wind.						Mean Weekly Number of Days of Haze.		
No Cholera.	Cholera Increasing.	Cholera Declining.	Cholera Increasing.	Cholera Declining.	No Cholera.	Cholera Increasing.	Cholera Declining.	No Cholera.	Cholera Increasing.	Cholera Declining.	No Cholera.		Cholera Increasing.		Cholera Declining.		No Cholera.	Cholera Increasing.	Cholera Declining.
											N.	S.	N.	S.	N.	S.			
29·611	29·754	29·617	57·6	52·0	2·3	0·9	1·4	4·0	1·7	3·6	0·6	6·0	3·8	3·1	1·3	4·8	0·0	3·3	1·3

These results show that the atmospheric conditions during the progress and decline of the epidemic of last year (1866) were similar to those of former epidemics. The connection of cirri and halos with the decline of cholera is owing to these clouds being peculiar to the equatorial or ozoniferous current, and it is among *these* that halos and parhelia are formed.

The conditions favourable to the development of cholera poison seem to exist at the same time at great distances. In the week ending June 30th seventy-three deaths from cholera and choleraic diarrhœa occurred in London, and in the following week thirty-four were registered in Liverpool; and in the same week in which there was a decrease of 362 deaths in London, there was no increase in the number of deaths in Liverpool; in the following week the disease was declining in both towns. In the week ending September 8th the disease continued to decline, but in the week ending the 15th there was an increase in the number of deaths at both places. During the two following weeks the number of deaths again decreased, but in the weeks ending 6th and 13th of October it again increased in both towns, and it appeared at North Shields, Sunderland, Chester, and in towns and villages in Flintshire and Devonshire; and in the same and following weeks the deaths from cholera in Dublin were 81, 98, and 118.

The meteorological conditions under which cholera increases and declines also prevail at the same time at great distances. From June 1st to the 23rd the mean daily amount of ozone at Carlisle, Miltown near Dublin, and at Hawarden in Flintshire, was 4·0. In the week in which cholera commenced at London and Liverpool the mean daily amount of ozone at the above stations was 1·7 only; and as cholera increased ozone was, at these places, in minimum quantity, and as it decreased ozone was in maximum quantity.

Cases of diarrhœa occur with changes of weather, and, as a rule,

sporadic cases of cholera are accompanied by storms and gales. In the following table I have given the highest reading of the barometer on the day before, and the lowest on the day on and the day after which diarrhœa occurred, and the mean amount of ozone and the force of the wind on these days, by which it appears that there is a decrease in the readings of the barometer on the day on which diarrhœa occurs and on the day after it, and that there is an increase in the quantity of ozone and the force of the wind on these days.

TABLE V.

Highest and Lowest Readings of Barometer.			Mean of Ozone.			Mean Force of Wind.		
Highest. Day before Diarrhœa.	Lowest. Day of Diarrhœa.	Lowest. Day after Diarrhœa.	Day before Diarrhœa.	Day of Diarrhœa.	Day after Diarrhœa.	Day before Diarrhœa.	Day of Diarrhœa.	Day after Diarrhœa.
29·724	29·635	29·599	1	2	1	1·0	2·0	1·6

TABLE VI.

Showing the Highest Reading of the Barometer on the day before Sporadic Cholera, and the lowest on the day after it; and the Force of the Wind and the Amount of Ozone on those days.

Reading of Barometer.		Quantity of Ozone.		Force of Wind.	
Day before Sporadic Cholera.	Day after Sporadic Cholera.	Day before Sporadic Cholera.	Day after Sporadic Cholera.	Day before Sporadic Cholera.	Day after Sporadic Cholera.
29·367	29·007	2·8	6·0	2·4	3·4

From March 28th, 1862, to March 11th, 1864, that is during a period of two years, 60 per cent. of the cases of sporadic cholera occurred at the same time as the issuing of Admiral Fitzroy's cautioning telegrams.

These attacks of diarrhœa I attributed to vicissitudes of weather, and with the view of ascertaining the effects of atmospheric changes upon the action of the skin I made a long series of observations and experiments on evaporation of a saline solution through human skin and other membranes, and upon osmose, in connection with atmospheric conditions.

The instrument with which I made the experiments is for measuring the quantity of evaporation through human skin or other membranes, in connection with atmospheric conditions. It is also a measurer of osmose.

It may be used either as a Diadermameter (*Δια through, δερμα skin, μετρον a measure,*) or a Diadermosmometer (*Δια, δερμα, ωσμος impulsion, μετρον*). It consists of two strong glasses, graduated from above downwards into inches and tenths of inches, and ounces and drachms. The glasses are fixed upon a mahogany stand. There is a thermometer on the stand, between the glasses, which is read at each observation. In each glass there is a glass tube, expanded to a quarter of an inch of surface at the lower end. To the upper end is fixed a vulcanite cup, presenting one square inch of surface. The lower surface of the vulcanite cup, external to the glass tube, is grooved so as to fit the lip of the measure-glass, and so prevent evaporation in that direction. When in action the No. 2 graduated glass is filled to the first mark with artificial serum, or a saline fluid of 1·030 density, and the tube with the vulcanite cup, covered with human skin or ox or pig bladder, is filled with the same fluid and inserted into the graduated glass. In doing this care is required lest air-bubbles be in the tube. As the fluid evaporates through the membrane it falls in the graduated glass, and the quantity lost is registered every morning at nine o'clock, with the readings of the thermometer and barometer, the direction of the wind, and its force, etc.

As the strong graduated glass was not graduated according to the space occupied by the tube it holds, I apply a correction for the latter; but in future I shall request that the glass be graduated while in *working order.* When it is wished to measure osmosic force with the quantity of cutaneous exhalation in connection with atmospheric conditions, fill the tube in No. 1 glass with the same fluid as that in the glass and tube No. 2. Cover the lower extremity of the tube (in No. 1) with a piece of the stomach of a pig or other membrane, seeing that there are no air-bubbles in it. Fill the glass No. 1, up to the first mark, with a fluid of *less* density (distilled water) than that in the tube. By comparing the quantities lost by evaporation, or the amount of decrease in height of the liquid in each graduated glass, the quantity of *en*dosmose will be ascertained. For instance, if glass No. 2 have lost two-tenths of an inch, and the glass No. 1 five-tenths, the three-tenths must have passed into the tube by endosmose. If the No. 1 graduated glass contain a *denser* fluid than that in the tube, the liquid in the glass does not fall so much as that in glass No. 2, because it gains from the tube by exosmose (such is the action of aperients). The membranes closing the tube become concave or convex according to the pressure, temperature, and degree of humidity of the atmosphere.

Table VII.

Showing the Mean Quantity of Liquid evaporated through a square inch of Human Skin Monthly.

	January.	February.	March.	April.	May.	June.	July.	August.	September.	October.	November.	December.
		drs.			drs.			drs.			drs.	
Mean monthly evaporation		6			10			12			5	

If we take 2,500 inches as the number of square inches of skin of a man of ordinary height and bulk, and if he were placed under similar conditions, he would give off by his skin eighty ounces of liquid daily. That, however, is greater than the estimated quantity given off during life. The amount of exhalation varies with the pressure, temperature, and humidity of the atmosphere, and the force of the wind. It is greatest when the pressure and temperature of the air are above the mean, and when the humidity is below the mean. If, however, the temperature be high when the air is saturated with moisture, it would appear that more *solid matter* passes off by the skin than when there is less degree of humidity with the same degree of temperature.

I made observations on the quantity of solids and liquids given off by the kidneys, at the same time. The quantity given off varies with the season, as shown by the following table.

Table VIII.

Showing the Quantity of Liquid evaporated through a square inch of Human Skin, and the Amount of Solids and Liquids given off by the Kidneys.

	January.	February.	March.	April.	May.	June.	July.	August.	September.	October.	November.	December.
		drs.			drs.			drs.			drs.	
Mean monthly evaporation		6			10			12			5	
		ozs.			ozs.			ozs.			ozs.	
Monthly mean of Solids.....		86			84			80			107	
		ozs.			ozs.			ozs.			ozs.	
Daily mean of Liquid.....		76			72			72			85	

Table IX.

Showing the Quantity of Liquid given off by a square inch of Human Skin, with Readings of the Barometer and Thermometer, and the Degree of Humidity of the Atmosphere.

	Mean of Barometer.		Mean Temperature.		Mean of Humidity.	
	Above.	Below.	Above.	Below.	Above.	Below.
	drs.	drs.	drs.	drs.	drs.	drs.
Mean monthly quantity of evaporation	8	7	10	5	7	8

Table X.

Showing the Mean Quantity of Liquid given off Daily by the Skin of an ordinary sized Man; also the Mean Daily Quantity of Liquid, and the Mean Monthly Quantity of Solids, given off by his Kidneys, with Readings of the Barometer, &c., above and below the mean.

	Mean of Barometer.		Mean Temperature.		Mean of Humidity.	
	Above.	Below.	Above.	Below.	Above.	Below.
	ozs.	ozs.	ozs.	ozs.	ozs.	ozs.
Mean daily quantity of evaporation through Skin of an ordinary sized man	83	73	104	52	73	83
Mean daily quantity of Liquids given off by the Kidneys	75	78	74	80	78	57
Mean monthly quantity of Solid given off by the Kidneys	75	90	60	120	90	75

Table XI.

Showing the Amount of Evaporation through the Skin of an ordinary Man, on the day before, the day on, and the day after, which Diarrhœa occurs.

Quantity of Liquid the Day before Diarrhœa.	Quantity of Liquid the Day of Diarrhœa.	Quantity of Liquid the Day after Diarrhœa.
Ounces.	Ounces.	Ounces.
82	75	89

The results of these experiments and observations show, that when cutaneous exhalation is interrupted by atmospheric changes or vicissitudes of weather, the functions of the mucous membranes and kidneys become vicarious.

In my observations on osmose I did not observe anything contrary to the well-known osmosic law; but when a tube, filled with a solution of salt, is closed at each end with membrane, and one end is immersed in plain water and the other exposed to the action of the atmosphere, the quantity of water absorbed into the tube (that is, the force of the current of endosmose) is determined by the quantity given off or evaporated through the membrane in contact with the atmosphere: a result which shows that the quantity of liquid absorbed (we might say into the blood) is determined, in some degree, by the quantity given off by the skin, and, consequently, by the conditions of the atmosphere.

While engaged with these experiments I noticed some remarks on the efficacy of the mineral acids, especially sulphuric acid, in the treatment of cholera and choleraic diarrhœa, and I performed some experiments with them and other medicines, with the view of ascertaining their effects upon osmosic action. Into a tube terminating in an expanded end, presenting a square inch of surface, and covered with a piece of the stomach of a pig, I put some artificial serum. The end covered with the piece of stomach I inserted in a solution of common salt. On exosmose being fully established I added diluted sulphuric acid to the solution of salt in the outer vessel, which perceptibly checked the out-going current: a result which was perhaps owing to the change of density of the saline solution, produced by the addition of the diluted acid. In ten minutes the artificial serum in the tube gave a decided acid reaction, showing that the acid had entered the tube through the stomach, contrary to the current of exosmose. I obtained similar results with diluted nitric and muriatic acids, with solution of muriate of morphia, and with solution of carbonate of soda, potassa, and ammonia. No albumen, however, escaped from the serum into the outer vessel when the acids and muriate of morphia were used, but with the alkalies it escaped freely.

Having observed then that epidemic and sporadic cholera prevailed under opposite conditions of the atmosphere—the one produced by a special poison, and the other by vicissitudes of weather—and believing that the evacuations in the epidemic form are the result of exosmosis, I was induced to try ozone as a disinfectant, and sulphuric acid as a remedy.

PRACTICAL DEDUCTIONS.

As it appeared then that cholera epidemics declined and disappeared under the influence of the ozoniferous current of the atmosphere, I was induced to try ozone, artificially formed, as a disinfectant; and as it can be conveniently and cheaply formed by the action of phosphorus upon the atmosphere, I used that substance for producing it.

Ozone is a powerfully oxidizing disinfectant; it readily removes all noxious vapours arising from the decomposition of animal and vegetable bodies, and destroys poison germs. The products of putrefaction are not only favourable to the development of the germs of special poisons, but they by their continued action prevent the proper oxidation of the tissues, and thereby produce a predisposition to the action of poison germs. Ozone, diffused in a gaseous form throughout apartments, not only disinfects by removing noxious vapours and poison germs, but, by being introduced into the blood through the lungs, it oxidizes the effete matters produced by destructive assimilation, or the renovation of tissue, and thereby produces a degree of resistance to their action in the animal economy.

My experience with ozone as a disinfectant in fever is limited; but in the cases in which I used it as such, it seemed effectually to check the progress of infection. During the epidemic of last year (1866) all the cases of cholera which came under my notice occurred in nine houses. In six of these I used carbolic acid and Condy's fluid as disinfectants, and, in four of the six, more than one case of cholera occurred. In three houses I used carbolic acid, Condy's fluid, and phosphorus—that is, ozone—and one case only occurred in each house. I merely state the facts.

In cattle plague I used ozone—that is, phosphorus—extensively as a disinfectant. My observations extended over a period of six months, and they were made upon 267 stocks, consisting of 2,783 animals. Of the *stocks* that were not disinfected 51·8 per cent. were diseased, and 11·3 cattle died weekly. Of the *stocks* that were disinfected 27·2 per cent. were diseased, and 9·0 animals died weekly. Taking the per-centages of the *cattle*, we find, of those that were not disinfected 70·0 per cent. were diseased, and 56·8 per cent. died; while of those that were disinfected 40·0 per cent. were diseased, and 33·0 per cent. died. These results are from stocks that were scattered over an area of five miles' radius, in which the system of disinfection did not receive the attention it required. Taking an area of one mile's radius, in which the process of disinfection was under my own supervision, the results

are much more favourable. In this smaller area there were, at Christmas, 1865, thirty-three stocks in perfect health; ten of these were put under my method of disinfection. On the 9th of May, 1866, 96·0 per cent. of the disinfected cattle were healthy, while of those that were not disinfected 39·1 per cent. only remained.

That ozone gives resistance to the action of cattle-plague poison is evident from the following facts. In a locality there were four stocks, within about half a mile of each other, consisting of sixty, twenty-six, twenty-three, and twenty-two animals; the centre stock, consisting of twenty-three cattle, was disinfected. This stock was not only the last of the four which was attacked with the plague, but the disease lingered with it for seven weeks, while the other stocks were carried off in a mean period of thirteen days.

If there be any truth in these results, and if ozone possesses the disinfecting properties it seems to have, cattle plague ought to have been less prevalent and fatal in the western counties, in hilly districts, and where there are no manufactories, than in the eastern and midland counties, in districts of low elevation, and in manufacturing districts.

From the following numbers for the week ending March 3rd, 1866, it will be observed that such was the case.

Number of cattle attacked in south-western counties, Monmouthshire, and Wales	9,027
Number attacked in eastern counties and in Yorkshire ...	40,526
Number attacked in west-midland counties (inland).....	10,476
Number attacked in Lancashire (manufacturing) and Cheshire (crowded)	51,240

If the occurrence of the maximum and minimum of cases of cattle plague, with the conditions which afford the maximum and minimum of ozone, be only a coincidence, it is a very remarkable one.

The western counties of Scotland were all but free from the disease, and I am informed that it was seldom observed above an elevation of 500 feet, and that no case occurred in Yorkshire at a height of more than 1,000 feet above the level of the sea.

I have stated that in my experiments on osmose I observed that sulphuric, nitric, and muriatic acids, solution of muriate of morphia, and the carbonate of soda, potassa, and ammonia, entered a tube containing artificial serum by endosmose, contrary to the current of exosmose; and that although the acids and muriate of morphia entered the tube no albumen escaped, but with the alkalies it

escaped freely. Now, all who have examined the evacuations of cholera patients must have observed that they are highly alkaline, and are extremely prone to putrefaction; that those from the intestines contain numerous shreds of albumen; and that the urine contains albuminous casts of the tubuli uriniferæ. Whence this extreme alkalinity of the fluids? In health we know that the hollow organs possess sufficient vital power to prevent decomposition of their contents, and we know that whatever diminishes their nervous energy predisposes to decomposition of their contents, and produces alkalinity. May we not conclude then that the cholera poison produces at once such nervous depression, that alkalinity, with all its train of effects, rapidly follows? The alvine evacuations contain also numbers of vibriones (Dr. Hassall), and for the development of these an alkaline fluid and organic matter in a state of decomposition are highly favourable, if not absolutely necessary. If then there be any connection between the discharge of albumen from the blood and the alkalinity of the evacuations, and if the presence of vibriones have anything to do with cholera as a cause, the necessity for the administration of sulphuric or other acids is at once pointed out, for they not only neutralize the alkalinity, but they check the tendency to decomposition, and thereby remove the conditions so essential to the development of vibriones.

I had not an opportunity of testing the efficacy of the acid treatment of cholera until the epidemic of last year (1866). From August 7th to November 22nd, fifty-seven cases of diarrhœa, diarrhœa and vomiting with cramps in the extremities, and cholera, came under my immediate notice. Twenty-seven were cases of diarrhœa, seventeen were diarrhœa with vomiting and cramps, and thirteen were of Asiatic cholera. The cases of simple diarrhœa and of diarrhœa with vomiting and cramps were treated on general principles: all recovered. The thirteen cases of cholera, with two exceptions, were treated solely with sulphuric acid; the two exceptions were treated with opium, aromatics, and stimulants. One died. Four of the eleven that were treated with sulphuric acid died, giving 63·7 or nearly 64 per cent. of recoveries. I may state that two of the four who died under the acid treatment were women above seventy-four years of age; all the cases were in a state of collapse or extreme exhaustion when I first saw them. One woman (aged seventy-five years) died three hours after I saw her, and, even in her case, the vomiting had ceased, and the diarrhœa was checked under the influence of the acid.

In all cases I gave at once forty drops of the diluted sulphuric acid of the Pharmacopeia in two tea-spoonfuls of cold water. If the patient vomited in a quarter of an hour after the dose, it was at once repeated. If he did not vomit, it was not repeated until half an hour had elapsed, and so on until three doses were administered. After three doses the quantity was diminished to twenty

drops every half hour or hour until the symptoms were relieved. The patients were allowed cold water, acidulated with aromatic sulphuric acid, *ad libitum*, as a drink. As convalescence approached they were allowed, first beef tea, then milk and water, or milk. If pain continued after vomiting and purging had ceased, I gave muriate of morphia. In all cases alcohol was strictly prohibited; when given by attendants it invariably aggravated the symptoms, which I attributed to its osmosic action. The secondary fever I treated upon general principles, avoiding effervescing or alkaline drinks, allowing only beef tea, and milk, or milk and water.

SEPTICÆMIA;

WITHOUT AND WITH PURULENT DEPOSIT (PYÆMIA).

By A. WYNN WILLIAMS, M.D.,

Physician to the Samaritan Free Hospital for Women and Children.

THE subject I have selected to introduce to the consideration of my brother associates, is one which, at the present day is exciting much and deserved attention amongst the members of the medical profession; and if we look at the long and fatal list of cases given in the last number of The St. Bartholomew's Hospital Reports, by Mr. Savory, we need not feel surprised that a disease so generally fatal as is pyæmia, should engage the close and earnest study which the medical man is so ready and willing to bestow on any subject having for its object the warding off and ameliorating disease and suffering. I do not then doubt for a moment being able to engage your attention; but I do doubt my power to treat so interesting a subject with that ability which it deserves. If however I shall succeed in fixing your attention, my object will in part, at any rate, be attained.

I may commence my remarks by stating that the affection of which I am about to treat was known until very recently under the name only of Pyæmia. I have however, as you will perceive, selected, after deep consideration, for the title of my paper, Septicæmia, without and with purulent deposit; and I have done this advisedly, because I believe the formation of pus in different parts of the body to be an accidental circumstance; that this formation is in fact simply an effort of nature to get rid of a source of irritation, by isolating the noxious material, and surrounding it with bland pus, and that thus, by the well known rule by which pus invariably endeavours to find an exit through the skin or some internal channel, provision is made for its removal out of the system. This law is well exemplified in pustular diseases, or where any local irritant has been introduced under the skin, as a thorn, &c. Pure pus is of itself an innocuous material, and injurious only when it has undergone a chemical change known as putrefaction. It then becomes, as does any other animal matter after having undergone this chemical decomposition, a powerful poison of a most irritating and depressing nature.

We are all aware that the products of animal decomposition are certain compounds of carbon and hydrogen, hydro-carbons, which we will for convenience call carburetted hydrogen, and two other

compounds of hydrogen, phosphuretted and sulphuretted hydrogen. The carburetted hydrogen gas can scarcely be said to be injurious to life, except so far as it occupies the place of that essential to life, oxygen. Of the effects of phosphuretted hydrogen on animal life little is known. However, from its fœtid alliaceous odour we may readily believe, that, if introduced into the system, it would be most detrimental. The symptoms before death and the post-mortem appearances in cases of poisoning by phosphorus, and those of death from septicæmia, bear so great a resemblance to each other, that one cannot help being led to the inference that phosphuretted hydrogen plays an important part in cases of septic poisoning. Indeed, from the fact of so many cases of pyæmia occurring in patients suffering from necrosed or caries bone, one might well feel inclined to look upon it as more injurious than the third product sulphuretted hydrogen. This substance has long been considered as probably the most deleterious of all the gases to animal life. Air impregnated with only a very small quantity (1-150th) being sufficient to kill a horse, when inhaled by him. It has also the power of destroying rabbits, if the body only be immersed in it. I cannot enter into the particulars of the post-mortem appearances in cases of poisoning by sulphuretted hydrogen gas, but must refer you to Casper's and other works on medical jurisprudence to show how strong an analogy they bear to the appearances found after septic poisoning, especially in the destruction of the blood corpuscles.

The cause or the origin of septicæmia I believe to be the absorption into the circulation of the chemical products of decomposing animal matter, and their destructive influence on the blood corpuscles; and the secondary consequence, pyæmia or deposit of pus, to be merely the arrest of some portion of the decomposed blood in some of the tissues of the body, which acting as a local irritant soon becomes surrounded with pus, this again becoming decomposed, and a fresh centre for the transmission of the septic poison.

I have not time to discuss minutely the different theories propounded as to the causes of pyæmia, still I must make some allusion to them. It was long supposed that pyæmia was caused by the introduction of pus into the veins; the pus globules thus introduced becoming the centres or germs, as it were, for the growth and formation of fresh pus globules. This theory is now almost exploded; it having been clearly shown by previous writers, that pure fresh pus introduced into animals is no more productive of injury then any other foreign material, and that it is only when the pus has undergone decomposition that its presence becomes injurious. Another theory is that of inflammation of the inner coats of the veins and embolism of the smaller arteries and capillaries, and subsequent disintegration. This theory of itself implies a previously diseased condition. The inflammation of the vein, and subsequent embolism must have had an exciting cause. It is argued that this may be caused by pus globules or coagula

in the process of disintegration. This also implies a previously diseased condition not accounted for. Another is, that of metastasis. But I think we shall find few who now believe in such an hypothesis. There is still another theory, that it is caused by the presence of small animalculæ or vibriones. These I believe to be generated after decomposition has taken place, and can only be the means of conveying the septic poison which may be adherent to them.

We see exactly the same sort of occurrence taking place in vegetable life, namely in the potato. The disease in this valuable esculent has been set down to the presence of small animalculæ. Some years ago I paid a good deal of attention to this disease, and for several successive years foretold its coming. I first perceived small black spots on the leaves and stems of the potato plant, which continued for several days before it extended to the adjacent parts. Ultimately it extended down the stems, thence to the tubers. If the tubers were exposed it would commence at once in them. I was then led to consider what could be the cause of the spots, and, after two or more years' observation, noticed that the disease made its appearance in from nine to ten days after a slight shower of rain, when the atmosphere appeared to be charged with an undue amount of the electric fluid. Whether the drops of rain were so highly charged with electricity as to be injurious to the life of the potato, or whether the rain drops were charged with some other deleterious agent, I know not: but this I do know, that the disease was always preceded by such a shower of rain. I have had, I might almost say, ocular demonstration of the fact. On a certain occasion I was caught in one of these showers, where the drops are very large, and comparatively speaking very few, with a particularly close and sultry atmosphere. From such a shower I took shelter under an American oak, and was induced to admire the splendour of its foliage. On going into the house I remarked casually that we should have the potato disease in less than a fortnight, and sure enough we had. I was not, however, prepared for the effects of that shower of rain on the tree under which I had taken shelter; for on passing the spot some fortnight after, I observed that all the external leaves of the tree—that is, those exposed to the shower of rain—were drilled full of holes, much the same as if you had thrown over them a quantity of red hot shot. In this tree nothing more than the local injury accrued, which I attributed to its non-succulent nature. After due consideration of these phenomena, I was led to the conclusion that the potato disease was induced first by local injury; the drops of rain, with whatever they were charged, being the injurious material; then decay or gangrene of the part injured, with the products of vegetable decomposition, (no one, I fancy, who has once walked through a field of diseased potatoes, can ever forget its most horrible stench,) and the absorption or conduction of this poisonous product by means of the circulation to the rest of the plant. In fact,

septicæmia; the animalculæ, or white looking powder, being the result, not the cause of the disease. In like manner the vibriones present in wounds, accompanied by septicæmia, are after products consequent on the decay of animal matter, and are not the cause of the septic poison.

Septicæmia, then, as before stated, I believe to be caused by the absorption into the blood of a poisonous material, probably gaseous and soluble in water, the product of the decomposition of animal matter, which is, moreover, always present in it; thus differing *in toto* from glanders, hydrophobia, small pox, syphilis, and the like, every one of which have a poison peculiar to itself, and not to be generated, so far as ascertained, *de novo*.

The poisonous matter of putrid meat, when introduced into the body by the mouth, produces, in the first instance, nausea, vomiting, diarrhœa, and sweating. Nature thus endeavours to get rid of the offending material by pouring out an extra quantity of serous fluid, for the purpose of protecting the mucous membrane of the alimentary canal, and then, by the vomiting, purging, and sweating, to expel it from the body. The introduction of septic poison in the shape of putrid meat into the animal economy does not always produce a like injurious effect. Thus, dogs and certain other animals can eat carrion with impunity, and well for man it is so, especially in India and other tropical climates; for were it not for these scavengers, the atmosphere would become so tainted as to be not only injurious but fatal to the life of human beings. Again, the effects of putrid meat are not always alike in human beings. Thus, one man may eat with impunity certain substances which another cannot touch. Indeed, nothing is more true than the old adage, "What is one man's meat is another man's poison." Thus, a man in strong robust health may eat a certain amount of putrid meat without producing any or very slight injurious effects, whereas another, in enfeebled and delicate health, would be quite overcome by the poisonous influence, and sink under it without timely assistance. Septic poison may be introduced into the system other than by the mouth. It may be, and indeed often is, introduced through a cut, scratch, or prick in the skin, as by dissecting wounds, producing a train of symptoms very similar to, if not identical with, those having their origin in a sloughing or putrid wound in the integument or some part of the body, a description of which you will find admirably given in Holmes's "System of Surgery."

There is, however, a form of blood poisoning occurring during the progress of various diseases, both acute and chronic, which is often, I fear, overlooked, or at any rate not set down to its proper cause, unless it should happen to be followed by one of the sequences of septicæmia; namely, the formation of pus or pyæmia, as, for instance, amongst the acute, small pox, scarlatina, diphtheria, puerperal and typhoid fever, and amongst the chronic, phthisis, chronic ulcers, strumous and other abscesses, &c. In fact, wherever we meet with

disorganisation and decomposition of animal matter, internally or externally, if there is not a free exit for the putrid emanation, there must be more or less septicæmia. It is true that in many diseases nature attempts to eliminate the poison through the skin and alimentary canal. In this way we must in part, if not entirely, account for the night sweats and diarrhœa in cases of phthisis, where we find vomicæ or softened tubercle. How often do we meet with phthisical patients living for years with large excavations in one or both lungs, the cavity or cavities freely communicating with the large bronchi, thus enabling the patient readily to eject the softened tubercle and decomposed lung tissue from the chest. When this is the case patients will go on living often for years, whereas another with, comparatively speaking, only a small portion of the tissue destroyed, and not having that free communication with the bronchi, will sink rapidly with night sweats, diarrhœa, &c., death having been due, not so much to the amount of mischief done to the lungs, as to the poisoning of the system by the gases formed during the decomposition of the pent-up tuberculous and other matters.

The evil consequences of the absorption of putrid emanations thus, as it were, meet and baffle the skill of the physician and surgeon at almost every turn. Time will permit me to do little more than allude cursorily to the means employed to counteract this evil influence. In every case, generous diet, tonic remedies, stimulants, fresh air, that is plenty of pure oxygen, and great cleanliness are demanded. These are essentials, for it is weakened and broken-down constitutions that are more readily attacked by septicæmia and pyæmia after accidents and surgical operations, and doubtless during the progress of other diseases. Other means also must be employed to prevent the putrid decomposition of the animal tissues, and for this purpose remedies and means are employed varying according to circumstances. Sir James Simpson advocates the employment of accupressure needles instead of ligatures in restraining the hæmorrhage from bleeding vessels, and is strongly backed by Professor Pirrie and others; and no doubt it must be an immense advantage to be able to constrict the extremity of an artery up to the point of death without causing its actual death and decay, thus reducing to a minimum the chances of septic poisoning from the strangulated extremity of the vessel. The silver suture is now in very general use as being less likely to retain any discharge that might become offensive. The actual cautery is recommended by others for the purpose of charring the extremities of vessels and surrounding tissues, especially in operations within the abdomen for the removal of ovarian and other tumours. My colleague, Mr. Spencer Wells, applies to the strangulate dend of the pedicle powdered perchloride of iron for the purpose, as it were, of tanning the tissues and preventing decomposition. Others again, and at the Middlesex Hospital apparently with great success, apply solutions of carbolic acid to the surface of wounds and other injuries as an antiseptic;

I can myself strongly recommend it as an excellent application in cases of small-pox. Solution of permanganate of potash, Condy's fluid, is used by others, but this certainly does not always succeed in removing even the smell. Others use the fumes of sulphur, sulphurous acid, for the purpose of causing speedy oxidation of the noxious products. To the offensive diphtheritic membrane I always apply a strong spirituous solution of tannic acid. This both tans the membrane and causes its immediate expulsion. Many other substances, too numerous even to mention, have been recommended. There are three, however, which require special attention, wood charcoal or carbon, chlorine, and iodine. The first, charcoal, is no doubt an excellent external application, and would seem to act beneficially through its strong power of absorbing and retaining gases; as an internal remedy where putrid meat has been swallowed it is most valuable, and also in correcting the fœtor of the breath. Chlorine has been in use for many years as a deodorizer and decomposer of putrefying animal and vegetable matter. Owing to its strong attraction for hydrogen it readily decomposes water, setting free oxygen, thus becoming an oxidising agent also. Useful as undoubtedly this agent is, it cannot be conveniently applied and kept in contact with the body, owing to the fact of its being a gas, and so readily combining with other substances. As an internal remedy a fresh solution has a most beneficial influence on putrid sore throats and other similar diseases. Iodine, although it has not so great an attraction for hydrogen as has chlorine and bromine, possesses far more practical advantages. In the first place, it is always ready to hand. If required for the purpose of purifying the atmosphere we have nothing to do but to procure some live coals on a fire-shovel, toss a few of the scales of iodine on the coals, when no sooner are the beautiful violet fumes seen to course through the room than the putrefactive odour is removed. The iodine, having robbed the noxious gases of their hydrogen, forms hydriodic acid, reducing to harmless and nearly inodorous substances the sulphur, the phosphorus, and the carbon, with which it was previously combined. The hydriodic acid, I may here remark, on exposure to air and light again gives up the hydrogen to the oxygen of the atmosphere, forming water; the iodine becoming again a free agent ready to exert its beneficial influence. If required for external application to a wound or otherwise, we have nothing more to do than mix a portion of the tincture with water, when we have a lotion ready for use, which can at once be applied to the wound, and the evaporation of the iodine outwards prevented by placing over it some impermeable cloth. Thus applied the heat of the body readily volatilises the iodine, so that it will permeate almost any extent of tissue, soon rendering the fœtid contents of abscesses inodorous. This then is the ingredient which I almost invariably order to be applied to every wound, suppurating or otherwise, especially when there is any putrefactive odour. Before opening an abscess I order,

if I have the opportunity, the application of the solution of iodine for a few days, and continue its use both by injecting and applying it externally as long as there is any discharge. It is quite astonishing how much the amount of discharge is lessened by this mode of treatment. In cases of phthisis I have seen more benefit derived from the application of a solution of iodine in water over the chest, or mixed with glycerine, and covered with oiled silk, than from any other means. These, however, need not be neglected. In this way I have treated every description of abscess, with results which, if witnessed by some of our operative surgeons, would cause them to hesitate before having recourse to the knife for excision of joints and removal of limbs. That iodine is a deodorizing agent no one can for a moment deny. Place a piece of putrid meat in a solution of it, and in a few seconds all taint is removed, and whatever is left may be eaten with impunity. This I have myself done on several occasions. After the immersion of the putrid meat the solution will be found to have become almost as black as ink from the deposit of carbon, sulphur, &c. That it removes the septic poison there can, I think, be no doubt. I injected some pus taken from an abscess behind the ear of a young child, after allowing it to become putrid and diluting it with water, into the side of a guinea pig. The pig sickened, and had diarrhœa for several days, and died on the eighth day. On examination after death the whole of the tissues were found to be softened and infiltrated with serous fluid. At the site of the injection there was found a quantity of what looked like pus in a state of decomposition. I injected at the same time and in like manner another guinea pig, but before injecting mixed a few drops of tincture of iodine with the putrid pus, removing all smell, and at the same time, as appeared by the result, destroying the septic poison; as the pig was in no way injured, with the exception of a slight ulcer at the seat of the puncture, and is now alive and well.* I injected another guinea pig with some offensive serous fluid taken from the abdominal cavity of a female who had died after tapping, and who was found after death to have been suffering from extensive cancerous disease of the abdominal viscera. This fluid was mixed with tincture of iodine before injection, and the pig has continued perfectly well. I shall now only relate the particulars of four cases, and that very briefly, to show the beneficial effects of iodine in preventing and neutralising the septic poison.

Case 1 is that of a patient, the particulars of whose case was

* It is worth recording that the second pig, although escaping the ill effects of the septic poison, succumbed to the original tubercular matter contained in the putrid pus; it died in about three months, and on examination, the liver, spleen, pancreas, renal capsules, and ceroical glands, were found to be infiltrated with tubercular matter in different stages of development, the lungs being healthy. It would appear from this that Iodine has no influence on the tubercular matter or poison, but on the septic only.

given me by our worthy President. He had a patient suffering from varicocele, whom he wished to have operated upon for the radical cure. Mr. Henry Lee was the operator; everything went off satisfactorily. There was, however, a large amount of extravasated blood, and one night during sleep the patient managed to scratch through the integument, thereby exposing the extravasated blood. The patient now ran considerable risk of pyæmia, and Mr. Lee proposed to lay open the swelling and scoop out the blood. Dr. Richardson, however, remembering what I had previously mentioned to him as to the properties of iodine, was desirous of using it in this case, which was accordingly done with the happiest result. The blood became disentegrated without putrefaction, and gradually oozed from the swelling without any injurious consequences.

Case 2 is that of a lady, a patient of my own, who was suffering from strangulated umbilical hernia, which not being reducible, the services of my friend, Mr. Curling, were requested, who operated with his usual skill. The stricture was divided, but as the hernia was of long standing, it was not deemed advisable to return it within the abdomen. On the second day after the operation, there were strong symptoms of commencing gangrene, which were only too fully confirmed on the following day. The stench was now becoming intolerable; Mr. Curling recommended the free use of the permanganate of potash. This had but very trifling control over the smell. Iodine was then applied. The atmosphere of the room at once became pure. The gangrene ceased to spread, and the slough separated; and although there was some trouble with abscesses burrowing about the abdominal walls, and diarrhœa, still under the continued application of the iodine solution and astringent remedies she made a good recovery, with of course an artificial anus at the umbilicus.

Case 3. A young woman consulted me about a swelling which she had over the left buttock. She was suffering also from aphthous condition of the mouth, diarrhœa, and night sweats, coughing up every morning a large quantity of very offensive pus. I found, upon inquiry, that some two years previous to my seeing her she had had a large abscess opened in the right groin, evidently a lumbar abscess, which after some time healed up. The present swelling, and the expectoration of offensive matter, would appear to have commenced some months after. There was moist rhonchus at the base of the left lung, with dulness, and evident disease of the lower dorsal vertebra. I at once ordered the spine and buttock to be covered with flannels wrung out of a strong solution of iodine, and these again with oiled silk, and to take, if required, bismuth, with compound kino powder. She also took, during the course of her treatment, chlorate of potash, with opium and bark, and iron and quinine. She did not require to take many of the powders, for in two or three days after the application of the iodine the matter coughed up was no longer offensive. The diarrhœa and night

sweats ceased. The aphthous state of the mouth began to improve, and, at the same time, the appetite and the spirits. I continued the application of the iodine for three or four days before opening the abscess in the buttock. This gave exit to an immense quantity of pus, but entirely free from any offensive odour. The pus continued to flow out pretty freely for some days, gradually becoming less, and latterly the discharge had an oleaginous appearance; at no time was the discharge offensive. The expectoration of pus ceased on the opening of the abscess in the buttock. On endeavouring to trace the course of the abscess, I found that the pus escaped from the pelvis through the great sciatic notch. I passed a long elastic catheter through the opening along the track of the psoas and iliacus muscles behind the diaphragm, to a level with the diseased lower dorsal vertebra; to this I attached a glass syringe, and injected daily through it a solution of iodine, continuing also its external application. The patient made an excellent recovery, and has continued in the enjoyment of good health ever since, now five years ago.

Case 4. That of an infant, aged three months, who was brought to me to the Samaritan Hospital, November 6, 1866, apparently suffering from inflammation of the bowels, for which she was ordered suitable remedies. At the second visit the mother informed me that the little girl had a swelling over the right shoulder; and that the child had not been well since she was vaccinated, a month previously. On examining the arm I found a large scab over the seat of the vaccination, and on removing this there flowed from beneath it a considerable quantity of offensive matter; on further examination I found another swelling on the inner side of the left thigh. I now looked on the case as one of pyæmia; ordered the seat of the vaccination to be well cleansed; the swellings on the shoulder and thigh to be covered with a solution of iodine in the usual way; to take a mixture containing chlorate of potash, hydrochloric acid, and cinchona bark; also a few drops of brandy in the mother's milk. On the 23rd, opened the abscess over the shoulder, and gave exit to a considerable quantity of creamy-looking, inodorous pus; introduced a tent into the opening; to continue the application of the iodine; to take the mixture and brandy, and also cod liver oil. November 30th.—The child takes the breast and the brandy freely; is very much bleached; very little discharge from the shoulder. Opened the abscess in the thigh, which had extended half-way round the limb; treated it in the same manner as that over the shoulder. A swelling was now observed about the right knee. This was also ordered to be covered with the iodine; to continue the same remedies. This treatment was persevered in until the 11th of December, when the swelling, which evidently communicated with the knee joint, was opened, giving exit to a large quantity of inodorous pus. The discharge from the shoulder had now quite ceased, and that from the

thigh was very slight; to continue remedies. She attended until the 18th of January, when she was discharged cured. But I had not yet done with my little patient. Her mother again brought her to me on the 15th of February, now suffering from hydrocephalus. The sides of the fontanelles and lateral sutures were widely apart. She was ordered half-grain and grain doses of iodide of potassium with ammonia, and to apply the iodine lotion to the head, enclosing it in an oiled-skin cap. This treatment was continued until the 26th of March, without much alteration in the size of the head. She now began to refuse the breast; was ordered a mixture of chlorate of potash, hydrochloric acid and bark, and to take a few drops of brandy in the mother's milk. On the 29th of March, I made two issues, one over each parietal bone, using for the purpose a strong solution of bromine, in which I dipped a piece of thick blotting paper cut to the requisite size, placing over it a piece of transparent gutta-percha sheeting.

From this time the child began rapidly to improve, and the head to diminish in size. On the 9th of April, I find I ordered an elastic band to be placed firmly round the head. She continued very much the same treatment, with the addition of a little steel wine, and was again dismissed cured on the 28th May; the latter weeks having been merely presented for inspection. She has continued well ever since, and is little the worse for her long suffering, with the exception of the right knee joint being somewhat stiff, the patella having been slightly displaced to the outer side. But even this is gradually improving.

ON THE USE OF THE SULPHITE OF POTASH.

By J. F. Nicholls, M.D.

I have for many years believed that zymotic diseases and cancer are caused by the action of either vegetable or infusorial germs. It is not my intention, however, to enlarge upon this subject now, but only to mention the fact that fungi have been detected in the fluid of the ventricles of the brain, in urine, in the fæces, especially in cholera stools, in the lungs, stomach, and intestines, and in syphilitic ulcers. Recently the announcement of the discovery of spores in the blood, in cases of rheumatism, by Dr. Salisbury, in America; and the statement by Dr. Schmidt that in the recent epidemic of fever in the Mauritius, in those cases that died he invariably found the lining membrane of the stomach and intestines covered with a multitude of very minute plants of a fungus, similar in growth to that found in the water of the Grand River; and that he could also detect them in the secretions, on the corner of the mouth, on the tongue, on the eyes, and sometimes on the surface of the skin of living persons suffering from fever, whilst in the secretions of entirely healthy persons they were not to be found;—have tended to strengthen my belief that we are on the eve of having the fungoid or infusorial theory of the cause of zymotic diseases satisfactorily proved. It will probably be found that some orders of zymotic diseases owe their origin to vegetable, others to infusorial germs.

During the same time I have held the opinion that amongst some of the compounds of sulphur, a remedy would be found as much a specific in these cases as it is in scabies. As a student, one of my day dreams, and I have no doubt many here have built similar castles in the air, was that I might be enabled to discover some compound of sulphur that would cure cancer, a disease to which in those days I more especially paid attention.

In October, 1863, within a day or two of its delivery, I read in one of the newspapers an extract from Mr. Henry Lee's introductory address at St George's Hospital, in which he mentioned the result of the experiments of Dr. Polli, of Milan, showing that putrid blood injected into the veins of dogs, in every instance but one, caused their death, but that the sulphites, when administered in large and repeated doses, entirely prevented any ill effects from the injection. It immediately struck me, were not these sulphites the compounds I had been so long seeking for? At any rate, I determined to test

their effect, and I will now state the result, in the hope that some of those who have greater opportunities will be led to try the sulphite of potash in a large number of cases. I should, however, first mention that I selected the sulphite of potash on account of the known superior activity of the other combinations of the same base. The rationale of its action probably is that decomposition takes place, caused by the acid secretions of the stomach, and that the sulphurous acid set free either destroys the vitality of the germs in the stomach and intestines, or its beneficial effect may be caused by its entering into the blood, and preventing the development of the germs, and consequently the disintegration of the albumen in the blood. That this decomposition does take place is fairly proved, not only by the simple experiment of adding a drop or two of dilute nitro-hydrochloric acid to a little of the sulphite of potash dissolved in water, when the sulphurous acid can be immediately strongly smelt; but also by the fact that, sometimes as soon as taken, it causes considerable distension of the stomach, and the eructations smell strongly of sulphurous acid. This distension in a few cases produced vomiting; I have not found it act on the bowels, but in the few cases I tested I found it caused the urine to become alkaline.

The first case in which I prescribed it (on October 10th, 1863), was that of a female servant who, after having felt unwell for several days, appeared, when I saw her, to be suffering from typhoid fever, which I supposed was caused by drinking water contaminated by sewage matter, and by sleeping in a room into which the emanations of a cesspool entered. She took the sulphite three times a day in half-drachm doses, and in six days was convalescent.

Syphilis Primaria.—On the 3rd of September, 1865, I was consulted by a Mr. T., a commercial traveller, who had an ulcer on the dorsum of the penis rather less than a shilling in size, circular, with hardened edges and base, with a multiple bubo in the right groin; he had never been affected before; had been under the treatment of a druggist for six weeks; believed he had not had any mercury given him, but iodide of potassium and sarsaparilla, with black-wash to the sore. I ordered him to take twenty-grain doses of sulphite of potash twice a day, which he commenced taking the following morning, and to apply water dressing to the ulcer, and a lotion of hydrochlorate of ammonia to the bubo; on the third day after the sore looked healthier and not so deep; on the sixth day it was very much smaller in size and level, the hardened edges nearly gone; on the eighth day it was quite healed, bubo smaller; on the tenth day bubo much reduced in size, and not at all painful, hardening of the cicatrix very trifling. I did not see him again for a week, when the hardening of the cicatrix was gone, leaving a small depressed scar, and there was not any trace of the bubo; he continued taking the sulphite of potash for a fortnight after the sore was healed. I have seen him from time to time for more than

two years after treatment, and he has never had the slightest trace of secondary symptoms. I have since treated seven other cases of pure Hunterian chancre on the same plan, except that one or two used a lotion of sulphite of potash instead of water dressing to the sore; perhaps the surface appeared to look cleaner, but I did not find any greater benefit from it. With but one exception, I have traced each case—five of them for eighteen months, and one for six months, after treatment—and not one of them has had any secondary symptoms. This result, I think it may be fairly assumed, is most successful, when compared with any other plan of treatment.

Syphilis Secundaria.—G. B., æt. 27, contracted primary syphilis in the spring of 1864; had a chancre, accompanied by a bubo, which disappeared without suppurating; was treated in Haslar Hospital for six weeks, mouth not sore; then did duty for three months; returned to Haslar Hospital, and was under treatment for nearly five months for secondary disease; was discharged from the Royal Marine Light Infantry in March, 1865, as unfit for further service, on account of syphilitic rheumatism; since that time has been getting worse, pain in his head and limbs has been so violent that he has hardly left his bed for months; gets no rest, is much emaciated. First seen by me on the 30th of September, 1865, when he commenced taking twenty-grain doses of sulphite of potash twice a day. October 4th, pain in his limbs rather relieved; pain in the head of a duller character. October 7th, pain quite gone from his limbs; pain less in his head; gets a little rest at night; feels stronger than he has done for months. October 11th, looks much better to-day; no return of pain in his limbs; only slight heaviness in the head remaining; can sleep all night; is astonished to find that the present rough weather does not affect him, as previously it caused so much pain that he could hardly turn in his bed; has slight conjunctivitis, which continued for a few days, and slight pustular eruptions, which gradually disappeared. He continued taking full doses of his medicine up to the 21st, when, as he appeared convalescent, he commenced taking ten-grain doses. October 28th, caught cold from getting wet through yesterday. Iris of left eye slightly affected, to apply a small blister to the temple, belladonna to the eyebrow. November 1st, iris much inflamed, irregular; to take two-grain doses of quinine every four hours, continue using the belladonna. The iritis continued about the same for four or five days, when it commenced to improve; on the 10th it was nearly gone, and the iris, which at one time had been very irregular, had nearly regained its normal state; there was slight dulness of vision, which rapidly disappeared; he had taken the quinine up to the 8th. On the 11th, felt a return of pain in his head last night; recommenced taking the sulphite of potash, which he continued up to the 20th, but the pain only lasted a few days. On the 20th, as he looked anæmiated and had a slight pustular eruption coming out, I ordered him two-grain doses of sulphate of iron, with fifteen grains

of sulphate of magnesia, twice a day, which he continued taking for a short time, since which, up to the present time, he has been in perfect health. Subsequently I treated another case of secondary syphilis, but of much less severity, on the same plan, with equally good result.

I have prescribed the sulphite of potash in a case of phthisis in the last stage; it appeared to diminish the quantity of expectoration, and to relieve the diarrhœa, but for a few days only. I also gave it in a case of variola; from the history of infection, I was able to diagnose the disease before the appearance of the eruption; it was an extremely modified case; there were faint marks of vaccination. A brother and sister who continued to reside in the house from which my case was removed, had the disease in a very severe form; one, I heard, died.

A short time ago I had under my treatment a case, which from the identity of the symptoms I could only consider to be one of ague; the cold, hot, and sweating stages regularly succeeding each other. This man had suffered in India from intermittent fever, and said that the present attack was exactly similar; he took the sulphite of potash, and in three days was quite well. I have recently treated with the sulphite of potash thirty-five cases of typhoid fever out of seventy-one persons inhabiting a row of houses. A canal, into which I believe several drains empty themselves, runs parallel with, and about fifty yards from the houses, which are from ten to fourteen feet below its level. The drinking water is good, and not contaminated with sewage matter. The drainage is perfect, and as the epidemic was entirely local, there being no other cases in the town,* nor any trace of its having been imported from a distance, I can only suppose that emanations from the canal were the cause of the outbreak. I should mention that these cases were confined to the non-commissioned officers and drummers on the permanent staff of a militia regiment, their wives and children, who are in comfortable circumstances. The following is the result in a tabulated form.

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* I have since heard that at about the same time there were two or three cases of typhoid fever in the county prison, which is within 200 or 300 yards of the row of houses inhabited by the cases under my treatment, and about the same distance from the canal as the row of houses. One case died. Subsequently there have been more cases, making a total of ten, with two deaths.

No. of Case.	NAME.	Age.	Date of Attack.	Commencement of Treatment.	Date of Convalescence.	Duration of Case.	REMARKS.
		Years				Days.	
1.	Joanna L.	9	Nov. 3	Nov. 10	Dec. 1	29	A very severe case; delirium, diarrhœa, rose-coloured spots, chest symptoms.
2.	Emma C.	8	,, 6	,, 10	,, 2	27	A very severe attack; diarrhœa, rose spots, delirious for days, chest symptoms.
3.	George C.	10	,, 6	,, 11	Nov. 28	23	A mild attack; diarrhœa slight, rose spots absent.
4.	John C.	4	,, 8	,, 11	Dec. 15	38	A very severe attack; diarrhœa, rose spots, chest symptoms, anasarca. This child had scarlet fever badly eight months ago, and anasarca subsequently.
5.	William L....	15	,, 9	,, 11	Nov. 29	21	A mild attack; diarrhœa, chest symptoms, no rose spots.
6.	James G.	52	,, 10	,, 12	,, 28	19	A very slight attack; no diarrhœa nor rose spots. Head principally affected.
7.	Jane C.	23	,, 10	,, 14	Dec. 1	22	A slight attack; no diarrhœa nor rose spots. Anxiety for her child prevented earlier convalescence.
8.	Eliza S.	29	,, 10	,, 12	,, 1	22	Mild attack; no diarrhœa nor rose spots. Rest much broken attending her child.
9.	Edwin P.	16	,, 11	,, 14	Nov. 27	17	Mild attack; no diarrhœa, had rose spots.
10.	William G.....	9	,, 11	,, 13	Dec. 6	27	Very severe attack; continuous diarrhœa, rose spots, chest symptoms.
11.	Edgar G.	4	,, 11	,, 13	,, 3	23	Much delirium at first, great tympanites, diarrhœa for a day or two, rose spots.
12.	Henry C.	2	,, 11	,, 13	,, 8	28	A very severe attack, accompanied by dentition; petechiæ which sloughed, very severe diarrhœa, convulsions.
13.	Marian L.	4	,, 11	,, 13	,, 19	39	A very severe attack; diarrhœa, rose spots. Was kept alive by nutritive and stimulant enemata.
14.	Ernest S.	7	,, 11	,, 12	Nov. 30	20	Rather sharp attack; delirium, rose spots, but no diarrhœa.
15.	James C.	27	,, 12	,, 13	Dec. 2	21	Had been suffering from rheumatism for some weeks previously; slight delirium, rose spots, no diarrhœa, head symptoms.
16.	Harry C.	5	,, 13	,, 14	,, 1	19	Mild attack; diarrhœa, no rose spots observed.

No. of Case.	NAME.	Age.	Date of Attack.	Commencement of Treatment.	Date of Convalescence.	Duration of Case.	REMARKS.
		Years				Days.	
17.	William C.....	4	Nov. 13	Nov. 14	Dec. 2	20	Sharp attack; diarrhœa, rose spots.
18.	James C.	3	,, 13	,, 14	,, 1	19	Mild attack; no diarrhœa nor spots. Had been taking tonics for a long time previous to the attack.
19.	Caroline L.....	6	,, 13	,, 14	,, 6	24	Severe attack; diarrhœa, rose spots, chest symptoms.
20.	Silas S.	32	,, 13	,, 14	,, 1	19	Not very severe case; head and chest symptoms most prominent, no diarrhœa nor spots.
21.	Elizabeth C. ..	17	,, 14	,, 15	,, 3	20	At the onset this promised to be a very severe attack; much delirium, but she speedily got better, little or no diarrhœa, rose spots.
22.	M. E. H.	5	,, 14	,, 15	,, 2	19	Sharp attack at first; diarrhœa, rose spots, slight delirium for the first three days.
23.	Joshua C.	47	,, 16	,, 19	,, 4	19	Sharp attack at first; diarrhœa, no spots observed.
		mos.					
24.	James C.	19	,, 17	,, 18	,, 4	18	Sharp attack at first; diarrhœa, rose spots.
		years.					
25.	Mary L.....	11	,, 19	,, 23	,, 8	20	Mild attack; not any diarrhœa, rose spots.
26.	Ernest P.	10	,, 19	,, 23	,, 7	19	Not very severe attack; slight diarrhœa, rose spots.
		mos.					
27.	Frank S.	14	,, 20	,, 22	Died Dec. 1st.		Had diarrhœa ever since he was weaned, two months ago; was teething; first prescribed for on the 22nd; had convulsions on the 25th; rose spots; erysipelas the day before he died; only took the Sulphite of Potash for two or three days.
		years.					
28.	Fred. M.....	3	,, 20	,, 22	Dec. 8	19	Not very severe attack; diarrhœa, rose spots.
29.	Kate M.	4	,, 20	,, 22	,, 8	19	Sharp attack at first; severe diarrhœa, rose spots.
30.	Fred. H.	6	,, 22	,, 27	,, 13	22	Mild attack; trifling diarrhœa, no rose spots.
31.	Fred. S.	9	,, 24	,, 26	,, 4	11	Mild attack; slight diarrhœa, rose spots.
32.	Lucy H.....	3	Dec. 1	Dec. 5	,, 18	19	Rather sharp attack at first; delirium, severe diarrhœa for two or three days, no rose spots observed.
33.	Jane H.	13	,, 2	,, 4	,, 17	16	Very mild attack; slight diarrhœa, not any rose spots.
34.	Ann C.	43	,, 4	,, 6	,, 17	14	Very mild attack; slight diarrhœa, not any rose spots.
35.	Ann H.	25	,, 7	,, 8	,, 21	15	A mild attack; slight diarrhœa, not any rose spots.

These cases might be divided into three classes. First, mild cases; of these there were fourteen; nearly all had diarrhœa more or less, but rose-coloured spots were only present in three; they were all treated with the sulphite of potash in doses of twenty grains, in water, twice or three times a-day, for an adult; in proportionately diminished doses for children. The average duration of these cases was eighteen days; in one case there was a slight relapse. They all recovered. Second class, cases of greater severity; of these there were thirteen; ten had rose spots, ten severe diarrhœa, tympanites, and pain in the right iliac fossa. One case appeared to be more nearly allied to typhus, as there was neither diarrhœa nor rose spots, but severe head symptoms. In two of those who had rose spots, diarrhœa was absent, and two had diarrhœa but not rose spots. In seven there was a considerable amount of delirium for the first two or three days. The sulphite of potash was given in similar doses; there were two cases of slight relapse, but all recovered. In class third there were eight cases. All had severe diarrhœa, rose spots, chest symptoms, delirium, great prostration; two infants who were teething at the time had convulsions. The sulphite of potash was given in a similar manner. In six cases starch and opium enemata were given. The average duration was thirty days; seven out of the eight cases recovered. Nutrition was endeavoured to be well kept up with milk, beef-tea, arrowroot, wine and brandy, and by nutritive and stimulant enemata; in the more severe cases small quantities of nourishment and stimulants were given every half hour, and in some instances so continued for several days. The one case that died was that of an infant, fourteen months old, who had been suffering from diarrhœa ever since it had been weaned (two months previously). It was first treated on the 22nd of November for diarrhœa. On the 25th it had very severe and long-continued convulsions from dentition; on the following day rose spots came out, and on the day before its death, December the 1st, erysipelas appeared on the chin and spread over the neck. It only took the sulphite of potash for two or three days. I believe the long-continued diarrhœa and convulsions had quite as much to do with its death as typhoid fever, but I have thought it better to return its death as having been caused by fever, than let it for a moment be supposed that I had not fairly given the result of the treatment of these cases. It must be borne in mind that the child only took the sulphite for two or three days. The average death-rate of typhoid fever is about 1 in 6; if the death of the child is included, the mortality of these thirty-five cases is only 1 in 35. I think I may fairly assume that this small death-rate in cases of typhoid fever treated with the sulphite of potash is something more than a mere coincidence; at the same time, I well know how deceitful conclusions are when taken from the result of a small number of cases, and that only after a lengthened trial of the sulphite of potash can its true value be ascertained. So far, I am well satisfied with the result of its use in those classes of diseases in which I have tried it, and shall take every opportunity of prescribing it, not only in similar cases, but in cancer also.

ON TUBERCULOUS AFFECTIONS IN MAN AND IN THE LOWER ANIMALS, IN RELATION TO THEIR SUPPOSED ZYMOTIC NATURE.

By Edwards Crisp, M.D., M.R.C.S., L.A.C.,

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The subject of tubercle, both in man and in the lower animals, is one of the most important that can engage the attention of the medical practitioner.

The time allotted for the reading of this paper will not enable me to take such an extended view of the subject as I desire, but I have endeavoured, by the exhibition of a large number of drawings and preparations, to save words, and to make the matter clear to my hearers. In this communication I purpose confining myself chiefly to matters that bear especially upon the subject recently mooted, of the supposed zymotic and contagious nature of tubercle in man and in the lower animals, and I will first give an outline of the disease as it affects the human species, and then pass on to examine its nature and origin in the inferior mammals.

It was my intention to have been present at the late Congress in Paris, and to have taken part in the discussion on tubercle, but, although I was three times in that city during the Exhibition, I was unable to leave home when the Congress was sitting; a circumstance I much regret, as the exhibition of my preparations and drawings would have refuted many of the erroneous inferences respecting the infrequent occurrence of tubercle in the lower animals by M. Villemin, whose recent views on the nature and mode of propagation of tuberculous diseases have of late excited so much attention. In his work just published, "Etudes sur la tuberculose preuve rationelles experimentales de sa specificité et de son inoculabilité," which I have carefully read, M. Villemin makes the following extraordinary statement. "Mais il font avouer tout d'abord que les materiaux sont excessivement rare. En dehors de l'homme qui a malheureusement une aptitude spéciale pour la phthisie, il n'y a guère que le singe, la vache et peut être le lapin et quelque rongeurs analogues qui soit réellement susceptibles de devenir tuberculeux." (p. 484). And he moreover thinks that apes and monkeys do not become tuberculous from change of climate, but by an atmosphere vitiated by human beings.

These are questions that I shall have to consider hereafter, and I now pass on to notice such points connected with tubercular affections in man, that especially relate to the main subject of the inquiry, viz., the zymotic nature of tubercle, and its communicability from one person to another.

Among the chief causes of pulmonary tubercle may be enumerated climate and sudden changes of temperature; a moist atmosphere; the congregation of large bodies of people, and the consequent vitiation of the air by various contaminations; the deleterious effect of some trades and occupations; the influence exercised on the constitution by syphilis, open or concealed; and especially the hereditary transmission of the tubercular diathesis; more rarely the transmission of the actual disease, or the visible seeds of the disease, to the offspring at birth.

As I have to speak of the symptoms and morbid appearances of tubercle in the lower animals, it will be necessary to glance at them in man. Generally a peculiar aspect of body, eye, and complexion. In the ordinary pulmonary tubercle there is cough. Hæmoptysis is frequently present; in the report of the physicians of the Consumption Hospital, 1865, bleeding from the lungs was observed in 3,702 cases out of 5,516. More or less pain is felt in the upper part of the chest in some cases; the breathing is hurried on exertion; the pulse increases in frequency; rigors; gradual wasting takes place; purulent expectoration, night sweats, and diarrhœa; hoarseness, and sometimes aphonia; and the patient gradually dies from exhaustion. Other symptoms, too well known, may occur, and it is unnecessary here to allude to the stethoscopic signs.

The morbid appearances consist of various consolidations of the lungs; pleuritic adhesions; tubercular deposits, more frequent in the apices; vomicæ; cavities of various sizes; dilated bronchi; the bronchial and mesenteric glands are often enlarged, and tuberculous ulceration frequently takes place in the intestines, and also in the larynx and trachea; fatty liver is also frequently present.

Puckerings, cicatrices, fibrous contractions, and chalky deposits may exist in the lungs, which some deem indications of the former existence of cavities. In addition to the above, tubercles may be deposited in other organs, but these combinations are rare.

Although phthisis is one of the most common diseases that come under the notice of the medical practitioner, there is scarcely an affection that has occasioned more difference of opinion; microscopists differ as to its seat and the nature of the deposit; some find it in the air cells, others in the connective tissue, and a third set in the lymphatics.

One observer finds perfect cells; another declares that they do not exist; and a third sees a gradual cell progression and decline; and even chemists, whose deductions should be more perfect, disagree widely respecting the chemical composition of tubercle. The subject of climate has given rise to great difference of opinion; and

if I were to touch upon the diversities of treatment I fear the discrepancies would be tenfold. But let me briefly allude to some of the causes of phthisis that especially bear upon the question of its zymotic character.

In some countries, where men are living comparatively in a state of nature, with pure air, food, and drink, proper exercise, and the absence of mental disquietude and exertion, this scourge of the human race is comparatively rare; and it is tolerably certain that the coloured races are more exempt from it than the white, but it appears to be very prevalent among the mixed breeds. Probably there are no people on the globe that are exempt from this disease, and the supposed freedom of some races from this human scourge may have arisen from the general ignorance as to the cause of death among semi-civilized and savage races. The Arabs living in the desert are said to be free from it, and some African tribes are reported to enjoy the same exemption, but I am afraid that these statements are but little to be relied on. I believe the disease is on the increase in all European countries. In Europe the rate of mortality from this cause is excessive, and the disease is said to be on the increase. M. Villemin states that 150,000 die annually of it in France, and the rate of mortality in some of the European cities is not far short of this proportion! It is notorious, too, that in many localities to which consumptive invalids are sent, the fatality from this cause, among the inhabitants, is very great. But let us look to our own country, and see the localities that most favour the production of tubercle. According to the Registrar-General's Report, of which I have made an extensive analysis, the deaths registered in England and Wales, from 1851 to 1861, amounted to 4,210,715; of these, 508,923 were from phthisis, the deaths of the females exceeding those of the males by 30,313; the phthisical cases forming about 1 in $8\frac{1}{2}$ of the whole number of deaths. But as 77,254 deaths are included under scrofula and tabes mesenterica, and as many infants and young people, whose bodies have not been examined, have died of lung affections combined with tubercle, it may, I think be fairly inferred that the mortality from this cause amounts to 1 in $6\frac{1}{2}$ or 7 of the total number of deaths. In addition to phthisis the deaths in these ten years from lung affections amount to 573,826, and here, as may be expected, the male sex preponderates to the number of 42,684.

I have made an analysis of the Supplement of the Report for 1864, and some of the conclusions, I think, are of great interest.

First, let me take the proportion of phthisical cases in relation to the population in the various divisions of England and Wales: next, that of the metropolitan districts; and then many of the cities and towns in different parts of England and Wales. The analys is is my own; I avoid fractions, as perfect accuracy is unattainable where the data are somewhat imperfect. The number of deaths from phthisis during ten years, 1851 to 1861, is divided by the amount of the

population of the district, and the subjoined is the result, the *highest numbers* of course indicating that the locality named is more exempt from the disease.

North Western Counties, 31; Monmouthshire and Wales, 32; London, 34; Eastern, 37; Yorkshire, 37; North Midland, 39; South Eastern, 39; South Midland, 40; South Western, 41; West Midland, 42; Northern Counties, 42.

Before a correct estimate can be made as to the influence of locality in the production of phthisis, it would be necessary to investigate the occupations of the people; the nature of their food; the the character of the soil; the height above the sea level; the prevalence of certain winds, and other questions, the solution or attempted solution of which would more than fill a large volume.

Let me now proceed to the 36 metropolitan districts:—St. Olave (Hospitals), 11; West London (Hospital), 17; St. Saviour's, 25; St. Giles's, 26; Whitechapel, 27; Chelsea, 28; Westminster, 28; St. Martin's-in-the-Fields, 28; St. George's East, 28; St. George's, Southwark, 30; Greenwich, 30; Holborn (Hospital), 30; Strand, 33; St. James's, Westminster, 34; Lewisham, 34; Kensington (Brompton Hospital), 35; East London, 35; Shoreditch, 35; St. Pancras, 36; Marylebone, 36; Kennington, 36; Clerkenwell, 37; St. George's, Hanover Square, 37; Stepney, 37; Poplar, 37; St. Luke's, 38; Lambeth, 40; Bermondsey, 41; Wandsworth, 41; Camberwell, 41; Bethnal Green, 41; Islington, 42; City of London*, 46; Rotherhithe, 47; Hampstead, 61.

Let me now select various cities and towns in England and Wales:—

Liverpool, 24; Anglesea, 27; Manchester, 27; Southampton, 30; Hastings, 30; Bristol, 31; †Garstang, 31; Devizes, 32; Penzance, 33; Ipswich, 33; Scilly Isles, 33; Brighton, 33; Windsor, 34; Newcastle-on-Tyne, 35; York, 35; Portsea Isles, 35; Haltwhistle, 35; Plymouth, 36; Pembroke, 36; Birmingham, 37; Woodbridge, 37; Isle of Wight, 37; Lymington, 37; Louth, 37; Worcester, 37; Norwich, 37; Bath, 37; †Billesden, 38; Gravesend, 38; Truro, 38; Clifton, 39; Yarmouth, 40; New Forest, 40; Wolverhampton, 41; Dorking, 41; Hull, 41; Aylesbury, 42; †Longtown, 42; Easthampstead, 43; Falmouth, 43; Eastbourne, 43; †Bellingham, 44; Tynemouth, 45; Richmond, 47; Croydon, 49; †Rothbury, 57; King's Norton, 58; †Glendale, 60; †Knighton, 61; †Tenbury and †Pershore, 98.

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* It must be remembered in estimating the deaths in the City of London, that a vast number of people classed as residents die in the suburbs.

† The towns where the general mortality is the least are marked with a dagger.

SCOTLAND.

Unfortunately the deaths from phthisis are not registered as in England. Under the head of "Tubercular Class" are included *tabes mesenterica*, *phthisis*, and *hydrocephalus*; these form 16·10 per cent. of the whole specified causes of death—371 deaths in 100,000 persons. The mortality is highest in town districts; thus, in 100,000 persons in each district, there were 261 insular, 300 midland, and 491 town districts. The deaths each year for ten years were 10,007, 9,077, 10,326, 10,265, 9,951, 11,212, 10,582, 11,429, 10,981, 11,445, making an increase of 6,023 for the last five years. Time does not allow me to show that the classification of hydrocephalus and tabes mesenterica with phthisis is highly objectionable, and that it tends to falsify the inferences. The sooner the English and Scotch returns are made to assimilate the better.

IRELAND.

Unfortunately no satisfactory evidence can be obtained from this country. The reports which commenced in 1864, extend only over certain localities, and respecting these no summary has been made. In the report of the statistics of disease in Ireland, April, 1861, (p. 131), in diseases of the respiratory organs, consumption was the most prominent, 2,650 being registered; 1,487 females, 1,163 males. The proportion to the total sick, 1 in 29.

In these returns there are some leading facts indirectly connected with phthisis, of great interest:—(1) That the death-rate corresponds to the density of the population; (2) That the general mortality in England and Wales is at the rate of 24·47 per 1,000 of the living of all ages; (3) In the healthy and more thinly populated districts it was only 17·53 per 1,000, whilst in large town districts the general mortality was 28·01 per 1,000. Taking the counties of England and Wales, the following are the most healthy: thus in these ten years the deaths to 1,000 living were in Surrey (agricultural part), 18; Westmoreland, 18; Sussex, 19; Lincolnshire, 19; Dorsetshire, 19. Whilst in the metropolitan part of Surrey it was 25; London, 24; Staffordshire, 25; Lancashire, 27; Yorkshire, 23; East Riding, 23; West Riding, 24; North Riding, 19. Some of the healthiest places are the following: Hampstead, 17; Bromley, Kent, 16; Easthampstead, Berkshire, 16; Dulverton, Somersetshire, 16; Tenbury, Worcestershire, 17; Pershore, Worcestershire, 17; King's Norton, Birmingham, 17; Billesdon, Derbyshire, 16; Garstang, Lancashire, 16 and 18; Bellingham, Northumberland, 14; Glendale and Rothbury, 15; Haltwhistle, Belford, Bampton, Longtown, and Bootle, from 14 to

17 ; Knighton, Wales, 16. It will be seen that the lowest rate of mortality was in Bellingham, Northumberland. It should be stated that these are places generally with a small population.

Let us now enquire whether phthisis in the above-named healthy districts is less frequent? The inhabitants of Hampstead, as will be seen by the figures, when compared with the other metropolitan districts, are peculiarly exempt from pulmonary consumption, and the general mortality is also very low, 17 per 1,000 ; the average mortality of London being 24 per 1,000. The same remark will apply to Pershore, Tenbury, Knighton, Glendale, Kingstown, Bellingham, Easthampstead, and Longtown; but if we look to Garstang, Haltwhistle, Wolverhampton, and especially to some of the metropolitan districts, as Bermondsey, Lambeth, and Rotherhithe, and to many other towns in England, we shall find that the general rate of mortality does not correspond with the fatality from phthisis, although, as a general rule, tubercle is less prevalent in healthy districts. But one of the most practical and useful facts gleaned from this enquiry is the inference that the places of resort for our consumptive patients, as Hastings, Penzance, Brighton, Swansea, Isle of Wight, Bath, Truro, Clifton, Falmouth, Teignmouth, Eastbourne, and others, have a far greater mortality from consumption than Hampstead; thus the mortality in the three first-named places is nearly double that of this metropolitan district. It must be observed, however, that the deaths from phthisis in some of these localities are increased by non-residents.

But let me now allude to a very important fact which has an especial bearing upon the question of the zymotic and contagious nature of phthisis. If we look to the statistics of zymotic diseases, as fevers, small-pox, &c., we find that there is a great irregularity as regards their prevalence ; thus, to take the deaths from small-pox in Scotland :—

"In 1855 and 1856 respectively, the deaths by this disease were 1,309 and 1,306 ; in each of the years 1857, 1858, and 1859, the deaths were 845, 332, and 682. In 1860 they rose to 1,495, but declined in 1861 and 1862 to 766 and 426. In 1863 small-pox again became epidemic, and destroyed 1,646 lives. In 1864 1,741 persons died from the disease. In 1865 and 1866 the deaths were 123 and 280 respectively. The Scottish Vaccination Act came into operation on the 1st of January, 1864."

Again, the mortality from scarlatina in England and Wales varies from 30,000 to 9,000 yearly.

If we compare the general rate of mortality from phthisis how different is the result! Beginning with 1852 and ending at 1865, the year when the returns are last completed, the annual numbers stand thus: 50,954, 54,918, 51,284, 52,290, 48,950, 50,106, 50,442, 50,149, 51,024, 51,931, 50,962, 51,072, 53,046, 53,744. Comparing the last seven years above-named with the first seven years, 1852 to 1858, the deaths in the last-named seven years exceeded those of the first by 3,984, so that the statements recently

made respecting the diminution of phthisis from sanitary improvements are incorrect. There is another circumstance that militates against the inference that phthisis is a zymotic disease, viz., the mortality at certain ages; thus the greater number of patients in all countries are between the twentieth and thirty-fifth years, and there is a tolerable uniformity in the rate of mortality at the different periods of life.

In addition to the statistics I have given respecting the climate of England and Wales, let me add a few more. Certain localities might probably be found in which the mortality from phthisis is even less than at Pershore. Dr. Maund, in his History of Sandown, Isle of Wight, 1867, says, "Only one death has taken place amongst the *resident* population of the town of Sandown during the last five years, and he attributes this exemption to the free circulation of air."

If time would allow I could quote the statistics of various authors as to the influence of climate in the production of phthisis; the works of Louis, Andral, Clarke, Martin, Boudin, Schönlein, Lebert, Hérard and Cornil, Villemin, Edwin Lee, and many others, afford plenty of information under this head. To speak generally, a dry and cold climate, as some parts of Canada, Norway, Russia, and Sweden, where the variations of temperature are few; or warm, dry climates, as Algeria and some parts of Australia, are the most favourable. In the Statistical Report of the Health of the Navy, 1863, p. 51, it is stated that phthisis is more frequent on the Mediterranean station than any other, with the exception of the coast of Africa. The average ratio of the total force for seven years was 6·5 per 1,000. In theMediterranean it was as high as 8, and on the West Coast of Africa it was 9·2. In the summer season the disease in these localities increases often with great rapidity. In the same report, p. 295, the ratio of mortality in phthisis and hæmoptysis was as follows per 1,000: Home, 6·6; Mediterranean, 8; North America and West Indies, 5·7; Brazils, 4·9; Pacific, 3·9; West Coast of Africa, 9·2; Cape of Good Hope, 4·8; East Indies and China, 5·7; Australia, 5·7; Irregular Force, 8·6; Total, 6·5.

From the last Army Report, 1865, I am unable to glean any satisfactory evidence. The mortality from pulmonary diseases of the Foot Guards has been as high as 21·6 per 1,000, nearly double that of the Dragoon Guards. As quoted by Craigie, ("Practice of Physic," p. 1,002,) the number attacked with symptoms of consumption among the British troops per 1,000 was, Jamaica, 13; Windward and Leeward Command, 12; Bermuda, 8·8; Gibraltar, 8·2; Malta, 6·7; Ionian Isles, 5·3; Canada, 6·5; Nova Scotia and New Brunswick, 7; Cape District, 5·5; United Kingdom, 6·6; Mauritius, 7·7."

It is not my intention to touch upon the question of treatment, but before concluding this division on climate, let me introduce a

case of great practical interest. I could mention many examples in my own practice to show the benefit of a sea voyage and change of climate in the early stage of phthisis, but the following will suffice. A young lady about fifteen years of age, one of nine children, who had lost five of her brothers and sisters from pulmonary consumption, and was the only one of the family left when she came under my care several years since. She had all the symptoms of incipient tubercle at the upper part of both lungs, hæmoptysis, dulness on percussion under both clavicles, quick pulse, &c. I advised that she should go to Australia, and her friends wisely consented. I saw this lady, in England, about eight years after she had resided in Sydney, and she was in the enjoyment of perfect health. She was married, but childless. Unhappily she died of diseased liver four years after I last saw her.

I introduce this case especially as an example of the arrest of phthisis in the early stage, and to show that no zymotic disease could be so checked in its progress by change of climate or by any other means. The father of this lady and three of his brothers and sisters died of phthisis.

HEREDITARY PREDISPOSITION.

The evidence in favour of the transmission of tubercle from parent to offspring is so abundant that I think but few men in the profession can be found to question it. M. Villemin, however, doubts the correctness of the conclusion, and quotes several authorities, whose limited number of observations scarcely warrant notice. Thus, Briquet, in 106 phthisical patients found it hereditary in 96; Piorry in 1-15th of his patients; Pridoux, 1-4th; Louis, 1-10th; Lebert, 1-6th; Rillet and Barthez, 1-7th; Briquet, 1-3rd; but Louis judged from 31 cases, Piorry, 54, and the others from a very limited number.

Let me quote from the last Report of the Physicians of the Brompton Consumption Hospital before alluded to. "Out of 6,167 cases 4,432 were traced to hereditary predisposition; of these 450 were doubtful cases of phthisis. In a total of 6,167 cases, husband and wife were affected only in 106 instances, but the interval between the death of each is not stated"—the most important question! I have made numerous inquiries among my medical friends and acquaintances, and can meet with but few examples where husband and wife have fallen victims to this disease. In my own practice I have met with but one instance, and here, probably, there was hereditary predisposition on both sides, but I have forgotten the answer. If the examples, too, where husband and wife have died of pulmonary consumption are inquired into, it will often be found that the interval between the deaths has been so long that no contagious influence could have been exercised, and in other instances hereditary predisposition will fully explain the occurrence.

As regards the communicability of pulmonary tubercle from one person to another. I have met with several members of the profession who believe that it is contagious. The gentleman with whom I passed my pupilage, the late Mr. Cream, of Long Melford, Suffolk, was of this opinion. I have put the following question, verbal and in writing, to a great many medical practitioners: Have you in the course of your practice, met with any evidence to lead to the supposition that phthisis is, under *any circumstances*, contagious? The great majority have answered in the negative. Some think that the breath of a consumptive person may act injuriously by depressing the vital energy of those inhaling it. Dr. Bree, of Colchester, says, "After a practice of thirty-six years I have known so many relatives of phthisical people, *who have slept with the patient*, afterwards die of phthisis, even where there was no blood relationship, as a wife or husband, that the belief is a settled one in my mind. Neither do I think that there is anything extraordinary in this. If the tubercular element is so minute as to be conveyed by the ovum to the offspring, why should it not be thrown off by sweat, and be absorbed by the skin?" Other correspondents are of the same opinion, and I could mention several continental physicians who believe in the contagious nature of the disease. The late Dr. Bright inclined to this opinion. I must confess, although I speak guardedly, and with some amount of hesitation upon this question, that, as far as my own experience and present inquiries extend, they are opposed to the belief in the contagious nature of tubercle in the human species.

BAD AIR AND DIET.

The argument used by M. Villemin and other advocates of the zymotic theory, that the body cannot generate a poison like tubercle, is, I believe, utterly untenable. Tubercle, both in man and in the lower animals, can be produced at will; and although, happily, we are not allowed to experimentalise upon the human species, facts are not unfrequently brought to our notice that fully prove the correctness of this opinion. To my mind they are more convincing than the results produced by the inoculation of tuberculous matter, the effects of which may admit of a different interpretation. Let me give two practical examples that have come within my own observation during the last few years, the one from the human species, the other from the lower animals, to show the production of tubercular affections by a vitiated atmosphere and unnatural conditions.

In 1859 I attended a boy two years of age, who died of tubercular disease of the brain, as ascertained by a *post-mortem* examination. In the same family a girl, twelve years of age, died of tubercular peritonitis of rapid progress. The case is published in the "Transactions of the Pathological Society of London," vol. xi., p. 107. The important point of the case is that this girl had one soft tubercle in the lung, and since I published it I have thought it not improbable

that the system was inoculated from this tubercle. There are, I believe, several similar cases on record, but time will not allow me to refer to them. The inquiry is one of great interest. A brother of this patient, living in the same house, aged nineteen, had all the symptoms of incipient phthisis, hæmoptysis and partial consolidation of the upper lobes of both lungs. A fortunate journey however, at my recommendation, to the south of Africa for three years has much improved his health; he has recently called upon me, and is now comparatively well, Two other children were very delicate, one with tubercular lungs and crooked spine, but a four years' residence in a rural district has wonderfully improved her condition. I had known this family for thirty years when in another part of London, and had been present at the birth of several of them. The father and mother, now living in the country, so far as I could ascertain had but little hereditary tuberculous taint; the father's brother died from pulmonary hæmorrhage with slight tubercular deposit. The father was a very respectable cheesemonger in excellent business, living for many years in the same house, in a healthy locality in the suburbs of London, but the peculiar construction of his dwelling, I believe, and the vitiated atmosphere from gas and animal matters, occasioned the tubercular disease in his children. The house, originally a private one, was converted into a shop, the floor of this shop being about two yards below the ordinary ill-ventilated sitting-room. This shop, lighted with several gas-burners at night, and in dull weather often during the day, contained, besides butter and cheese, rabbits, pork, and other animal matters. The elevated sitting-room, and to a less extent other parts of the house, were often disagreeably hot and oppresssive, so that a stranger could scarcely enter the room without feeling the unpleasant effect of the gaseous atmosphere. It is unnecessary to speak of the suggestions made for the better ventilation of these rooms. I will only remark that there are thousands in London and in other large cities who are in a more vitiated atmosphere than these children, and in whose bodies tubercles are generated from the same cause. I think that the injurious effects of gas (often in shops allowed to escape to a great extent) and its production of tubercle have not been sufficiently considered.

Another crying evil, and one I believe that often lays the foundation for tubercular disease, is the exposed state of our London steamboat piers and railway stations. Many of them are more dangerous as regards the effect of cold winds, than if they were entirely exposed. No proper waiting place is provided for the passengers, and many when heated and excited from running to catch the train or boat are exposed to a cutting wind and its ill effects. This is an evil that should be at once corrected by the government.

The example from the lower animals is the following:—

Some years since I had a healthy cock and three hens from the

country; after being for some time in a large yard, they were confined in a shady spot (not for the purpose of experiment) in a close place between a bank of earth and a brick wall, with a netting over the top; after a time they began to lose flesh, and on killing them two or three months subsequently they all had tubercular livers and spleens.

In this yard and garden there had not been any poultry for several years, and there could have been no tubercular contamination, but at the Regent's Park Gardens I have met with a hundred examples at least where foreign birds, originally healthy, have died of tuberculous disease. Yet M. Villemin denies the existence of tubercle in birds! He might with as much reason deny the presence of tubercle in the human subject!

It is not contended that tubercle in a quadruped, bird, or reptile, would occasion exactly the same symptoms and morbid appearances as in man. Glanders in the horse, M. Villemin frequently asserts, is more allied to tubercle in man than any other disease; but, as I shall have occasion to show hereafter, the symptoms and duration of glanders in the human species are very different to those in the horse. Small-pox in the sheep differs somewhat from the variolous affection in man, but, notwithstanding, it is essentially small-pox. And I may remark, as shown by the drawings and preparations before the Society, there is much better evidence of the presence of genuine tubercle in birds, than in many of the rabbits inoculated by M. Villemin and others.

Occupations.—I am unable to enter fully into this question, but in general terms it may be stated that quarrymen, needle-pointers, knife-grinders, flax-dressers, and others engaged in occupations that favour the admission of extraneous bodies into the air-tubes, are especially liable to tubercular deposit, although this form of tubercle differs generally from that ordinarily met with, consisting often of small hard nodules with absence of cheesy matter. Among the subjects of genuine tubercle, domestic servants and labourers form a large proportion. In the report of the Consumption Hospital before alluded to, the subjoined are the occupations. Bakers 64, bookbinders 17, bricklayers 109, butchers 35, carpenters 295, clerks and shopmen 394, coachmen 211, gardeners 82, labourers 539, mechanics 76, painters 105, printers 103, publicans 46, railway 38, sailors and watermen 74, servants 285, shoemakers 171, soldiers and police 103, smiths 89, teachers 42, tailors 145, weavers 11.

In the *Annales d'Hygiene Publique* 1857, M. Trébuchet, as quoted by M. Villemin, p. 305, gives the occupation of those dying in Paris 1857-1858, and the following is the estimate per 1,000 of the consumptive cases. Bakers and pastrycooks 6·25, coal and charcoal venders 4·05, joiners 6·44, servants 12·68, grocers 2·84, brushmakers 2·30, workers in copper 2·75, butchers, tripe-sellers, porkbutchers, poulterers, 4·286, coachmen and cabmen 10·68, *religieuses* 13·35, *journaliers* 19·13.

I scarcely need say that to arrive at satisfactory conclusions these statistics should be on a much larger scale, and a very rigorous estimate of the numbers occupying each trade instituted.

Syphilis and its mode of treatment, I believe, indirectly serve to swell the list of consumptive patients in this and other countries, but I have not time to enter into this important question.

Other matters, such as interlobular pneumonia, and a fuller description of the so-called grey tubercle, tubercular peritonitis, and tubercle affecting other serous membranes, I must pass over for the same reason.

TUBERCLE IN THE LOWER ANIMALS.

During a long period, for the purpose of preparing a work on the practice of medicine, I have been investigating the diseases of plants and of the inferior animals with a view to throw light upon human pathology and treatment, and the subject of tubercle is one that has especially engaged my attention. For a great many years I examined most of the animals that died at the Zoological Gardens, Regent's Park, as well as in some private menageries, for the special purpose of ascertaining the cause of death, and in this inquiry I had the advantage of knowing the nature of the food and climate of these animals in a wild state, and the unnatural conditions to which they were exposed in confinement.

In my work on the "Structure and Use of the Spleen," 1854, I have recorded the cause of death in most of the foreign animals in confinement that I dissected; as well as in a subsequent Essay, the causes of death of the animals dying at the Regent's Park Gardens from 1851 to 1860, and also in the Proceedings of the Zoological Society.

In both these works will be found numerous examples of tubercle in various mammals, birds, and reptiles; and the drawings and preparations before the meeting show it in more than one hundred animals, a circumstance that some may think rather confirmatory of the contagious nature of the disease, as these animals were for the most part in the same locality and in confined and small spaces.

As I have said before, M. Villemin believes "that tubercle is confined to the monkey, the cow, the rabbit, and a few other rodents." In a subsequent part of his work after reviewing the nature of the *pommelière* or pulmonary consumption in the cow, he denies the existence of tubercles in the sheep, believing that nodules and tumors produced by *strongyli*, *echynococci*, and other worms, have been mistaken for them! It is quite true that sheep when they have a wide range are but rarely affected with genuine tubercle, although verminous tubercles are very common in these animals; but when, as in the case with foreign sheep and goats, confined to a limited space, tubercle is not an uncommon affection.

So much has this important matter been neglected that in our various museums I am scarcely acquainted with any specimens of tubercle in the lower animals, and the same remark I believe will apply to those on the continent. On a recent visit to the museum at the Veterinary School at Alfort, near Paris, I was surprised to find that it did not contain a single specimen of tubercle, either in wax, plaster, or spirit.

In 1854, (fourteen years since,) I presented to the council of the London College of Surgeons the spleens of more than 300 different species of animals to be placed in the museum, many of which were tuberculated. Among them were the tuberculated spleens of several monkeys, as well as the following. Coati mondi tyara (*M. barbara*), rock kangaroo (*P. penicillata*), manges dasyure (*D. Mangii*), beaver (*Castorfiber*),agouti (*D.agouti*). Birds—spotted cuckoo (*C.glandarius*), globose curassow (*C. globicera*), guan (*P. pileata*), ring-necked pheasant (*P. torquatus*), spurred plover (*S. limosa*), common crane (*G. cinerea*), native companion crane (*G. australasiana*), crowned crane (*A. pavonia*), sacred ibis (*I. religiosa*), rhea (*Rhea americana*), mandarin duck (*A. galericulata*), carolina duck (*A. sponsa*). Reptiles—python (*Molurus*), and puff adder (*C. arietans*).

The above were chiefly from animals that died at the Regent's Park Gardens in one year, 1852, but they include a very small proportion of those affected with tubercle; for example, the ruminants, in which animals tubercle of the lungs is most frequent, never have tuberculated spleens in confinement, judging from about forty-six different species that I have examined. Many animals, as I have said before, have the lungs tuberculated, and the other viscera in a normal condition; others have the tubercular deposit only in the spleen, liver, or intestines; whilst in some the liver and spleen are both implicated.

But let me briefly run over the vertebrate series, and point out those most prone to this affection. I may premise that tubercle, as far as my researches have gone among the works of travellers, is almost unknown in wild animals that have a wide range. I have dissected all of our British quadrupeds and reptiles, and most of our birds and fishes, including often a great many members of the same family; and I have met with genuine tubercle in only one instance, and this was in the spleen of a wood-pigeon.

The London sparrows can scarcely be called wild birds, but about twelve years since a great many of these birds were found dead about the Zoological Gardens. I examined several of them and they all had tubercle in the liver. I have not heard of any similar mortality among them since; some I have recently examined were free from tubercle.

As is well known, quadrumanous animals in confinement are very subject to tubercle, but the prevailing opinion that nearly all of them die from tubercular lungs is incorrect; for of more than 250 that I have examined, including many of the anthropoid apes, about

one-third only were affected with tubercle, the greater number having died of pneumonia, a state of lung that often leads to tubercular deposit.

I have only examined a few of the large foreign bats that have been in confinement in this country, and none of them were tuberculous. Among the insect and flesh feeders (*carnivora*) tubercle is comparatively rare, but I have met with it in several, including the leopard (*F. leopardus*), dog (*C. familiaris*), some of the cats (*felidæ*), racoon (*P. lotor*), surrikate (*V. tetradactyla*), tyara (*V. tyara*).

Among the rodents (chiefly vegetable feeders) tuberculous affections are very common, and I have seen several examples among the vegetable-eating marsupials, as the kangaroos.

In ruminants it is very common in the lungs, and in the deer, antelopes, and *camelidæ*, I have met with a great many examples; but I have never found the spleen, an organ frequently affected in the fore-mentioned classes, tuberculous in a ruminant. Tubercle in the lungs of cows, as is well known, is very common in our London dairies.

The pachyderms, with the exception of the pig, are but rarely subject to tubercle. In the lungs and spleen of the horse the disease is sometimes present, but in the pig I have seen some remarkable specimens, some of which are on the table. Among these is the tuberculous spleen of a boar, an animal in tolerable condition; the spleen weighed 3 lbs. 1 oz., the natural weight of this organ in the hog being about five ounces.

Before I quit the subject of tubercle in mammals let me again allude more particularly to tubercle among the London cows, and the same remark will apply to those in Paris and in other large cities. Tubercle of the lungs is very common amongst them, but it has been observed, both in London and Paris, that those which stand near to the windows and get a purer atmosphere are comparatively exempt. On the table is a recent specimen from the gyall or Indian ox (*Bos frontalis*); the lungs are studded with tubercles varying in size from a nut to a walnut, the mesenteric glands are also enlarged and tuberculous. Yowett in his work on the Ox says, p. 490, "That one in three of the London cows is diseased; the milk in these cows has a bluish watery appearance:" he adds, page 413, "It is rare that the offspring of a consumptive cow is not also consumptive. I have known whole dairies destroyed by this hereditary taint." But it will occur to many of my hearers that this question is one of vast importance as regards the prevalence of phthisis in this and in other large cities, for it may naturally be asked, If tubercle can be communicated from man to the rabbit and probably to the cow, why may not the milk from tuberculated cows occasion the disease in young children? I have not time to enter more fully into this question on the present occasion, but it is one that demands the serious consideration of the government, and would be an additional reason for the abolition of our London cowsheds.

HÆMOPTYSIS.

There is one remarkable circumstance about pulmonary tubercle in the lower animals; hæmoptysis never occurs. I have questioned several men who have been the keepers of a large number of monkeys and apes for a great many years, but none of them have met with this symptom. I have seen it once in a large lizard (*Uromastix spinipes*) with tuberculated lungs, but the keeper had put it into a warm bath, a most unnatural mode of treatment for a cold-blooded animal. Another fact should be named, viz., that tubercle in the lower animals, as in man, is not more confined to the upper lobes of the lungs, but is generally distributed.

BIRDS.

In birds, the liver, spleen, and intestines are more frequently affected with tubercle than the lungs, although these organs are often the seat of the disease; the same remark will apply to reptiles. In taking into account the diseases of birds and reptiles we must not forget the high temperature and quick circulation in the former, and the cold blood and slow-beating heart in the latter.

In birds in confinement tubercle is very prevalent, and, as in the lower mammals and in reptiles, they may be in good condition and even fat, when the liver, spleen, and intestines are one mass of disease. In cranes (*gruidæ*) and ducks (*anatidæ*) I have seen some of the most remarkable examples of this disease. Let me briefly notice the fresh specimen on the table from the brown crane, and when the parts are examined the question will naturally be asked, How did this bird survive with such an amount of structural lesion, such a destruction of function the continuance of which is considered necessary to life? The liver is studded with hard, tubercular masses of uniform size about the shape and magnitude of peas, so that no part of its structure is intact. Among the larger tubercles are small round granules, incipient growths that would attain the size of those first described (*Plate* 1, *fig.* 8). The spleen is enlarged, hard, and cheesy, like the liver, no part of its normal structure remaining. The peritoneal surface of the intestines is studded with small round tubercles of the same consistence as those in the liver (*Plate* 1, *fig.* 13). The lungs, as might be supposed, were free from tubercle, or the bird would not have survived. What interesting physiological inferences may be drawn from this case! How completely it upsets the blood-forming and blood-destroying theories of Kölliker and Bennet! But I have not time to indulge in physiological speculations. The blood corpuscles in this bird, as in many similar instances that have come to my

notice, were irregular in shape, narrow and yielding in form when in motion; a great many were without nuclei. The blood contained also free nuclei, nuclei, and granular matter (*Plate* 1, *fig. f*). In some birds as in mammals the lungs only are tuberculous, in others the spleen, and in a third division the liver and intestines, and in other examples all these organs may be implicated.

As among mammals, the strictly carnivorous birds, as the *falconidæ*, are not so often affected with tubercle as the vegetable feeders; but I have seen many of the *grallæ* (waders), and other feeders on mollusca, die from this cause. All the *gallinæ* (poultry birds) in close confinement are very subject to it, and they are all vegetable feeders.

The good condition of the bird when tubercles affect its vital parts is not a condition confined to this class. Oxen and pigs often fatten in the first stage of tubercle, and Bakewell, a member of our profession, the celebrated breeder of sheep, it is said always fluked his sheep before fattening them, the irritation occasioned by the parasite in the ducts acting in the first instance as a wholesome stimulus.

It must, however, be borne in mind that tubercle in the lower animals is of a harder nature, that large cavities are not generally formed in the lungs, and that the same exhaustive, purulent, bloody, and cutaneous discharges do not take place as in the human subject.

Tubercle in birds generally contains a large amount of cretaceous matter, so that when dried it assumes almost a stony hardness, as will be seen by the specimens on the table. I have in several cases, as I stated many years since, met with mould on the surface of false membranes and tubercular deposits in birds, which must have existed some time before death (*Plate* 1, *fig.* 14).

REPTILES.

The most remarkable form of tubercle is that affecting serpents and some lizards in confinement, and this grade of the disease I have reason to believe is highly contagious. I have known the great majority of the serpents in the reptile house at the Zoological Gardens killed by it, and if a fresh serpent is put into the same cage, unless very great care is taken, it is very likely to take the disease. I have reason also to believe that the disease makes rapid progress, and that it may be produced in serpents that are put into a close and ill-ventilated box during a sea voyage, as many serpents have it when they arrive in this country. I have examined a great many of these reptiles after death, that have died of this affection, and have watched them during life. The animal first becomes dull, and refuses its food; on looking at the mouth, which is often kept open, vascular spots are present, and at a more advanced stage tuberculous, cheesy matter is seen to be deposited in a

thin cyst; of these cysts there may be several or only one. As the disease advances, the head is elevated and the mouth is kept open; the animal generally dies, but in some cases recovery takes place. In addition to the morbid appearances in the mouth, tubercles are frequently deposited in the lungs, liver, spleen, and pancreas, and often under the integuments. The tubercles are generally of small size, at first grey and semi-transparent, and afterwards opaque and more or less cheesy; the most remarkable are seen in the intestinal canal, where tuberculous masses often as large as walnuts entirely block up the tube; these are more frequently seated in the rectum. I have placed several specimens of this form of tubercle on the table. I have but little doubt, as I have said before, that this disease in the serpent (and I have seen it in the lizard) is contagious, but I am performing experiments now that will I hope set the matter at rest.

I have recently examined after death a young rabbit and a guinea pig that would have been used for feeding the serpents at the Zoological Gardens. They were both in a normal state and free from entozoa.

If it could be shown that the animals used for feeding these serpents were tuberculous, it might throw some light on the origin of the disease; but I believe, like ordinary tubercle in man and in the lower animals, as well as glanders in the horse, it may be produced by exposure to a vitiated atmosphere, and to other unnatural conditions.

If it can be positively established that this form of tubercle is contagious it may go far to shake our faith in the non-contagious nature of human tubercle.

The microscopical appearances much resemble those of ordinary tubercle, as the drawings before the Society show. In the early stage, hyperæmia, lymph corpuscles, epithelial scales, imperfectly formed cells, granules, cholesterine plates, and cheesy and fatty matters are seen.

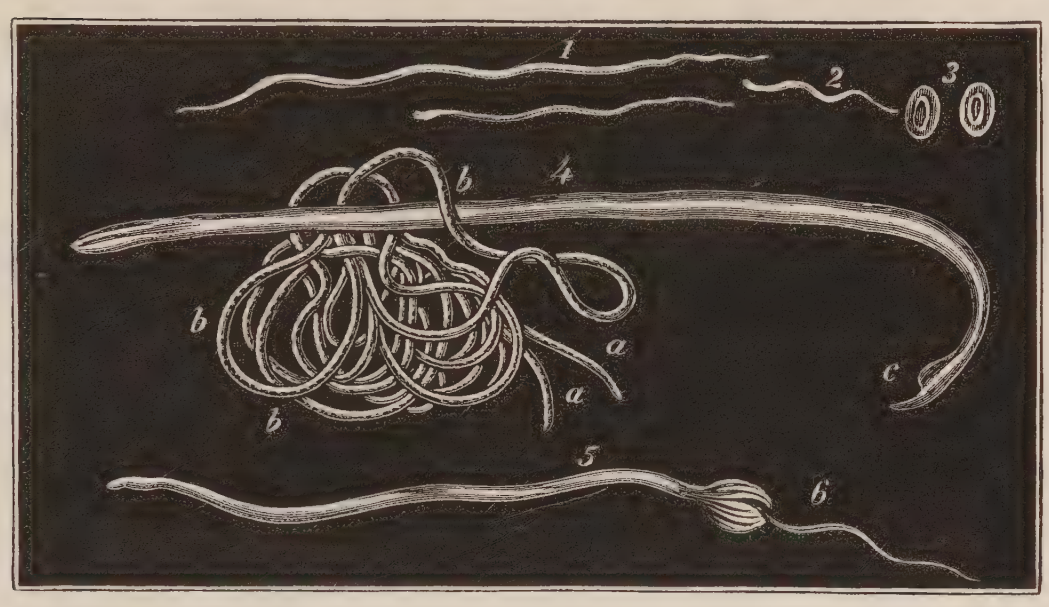
In several of the lizards I have met with tuberculous formations in the lungs, liver, and intestines. I have not, however, seen tubercle present in a batrachian. In the *Loricatæ* (alligators and crocodiles) I have seen several examples of tuberculous deposit, especially in the liver.

VERMINOUS TUBERCLE IN THE LUNGS OF SHEEP AND LAMBS.

This disease, which might readily be confounded with genuine tubercle, as shown in my Prize Essay on this subject, consists of tubercle-like nodules in the lungs filled with the ova and young worms of the *Strongylus filaria*. This disease is one of national importance, for tens of thousands of lambs are annually killed by it. It is especially important as regards the question of contagion, because Mr. Colin has produced tubercle in rabbits by inoculating

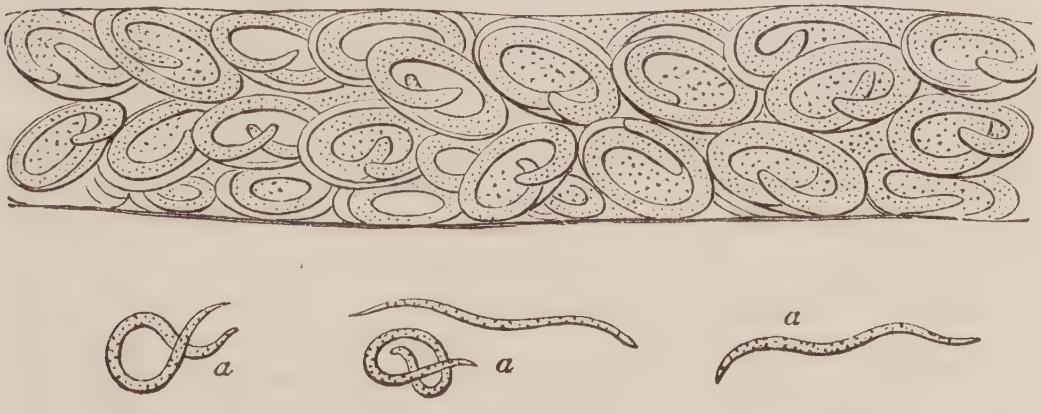
them with the matter of this false tubercle. The subjoined drawings which I have made of the worm in its various stages, and the magnified representations before the Society, will fully explain the matter. The first wood-cut represents—1, the female worm of the natural size; 2, the male; 4, the female, and 5, the male worm, magnified; 6, the bursa of the penis; 3, *a*, and *b*, the ova and young worms.

Fig. 1.



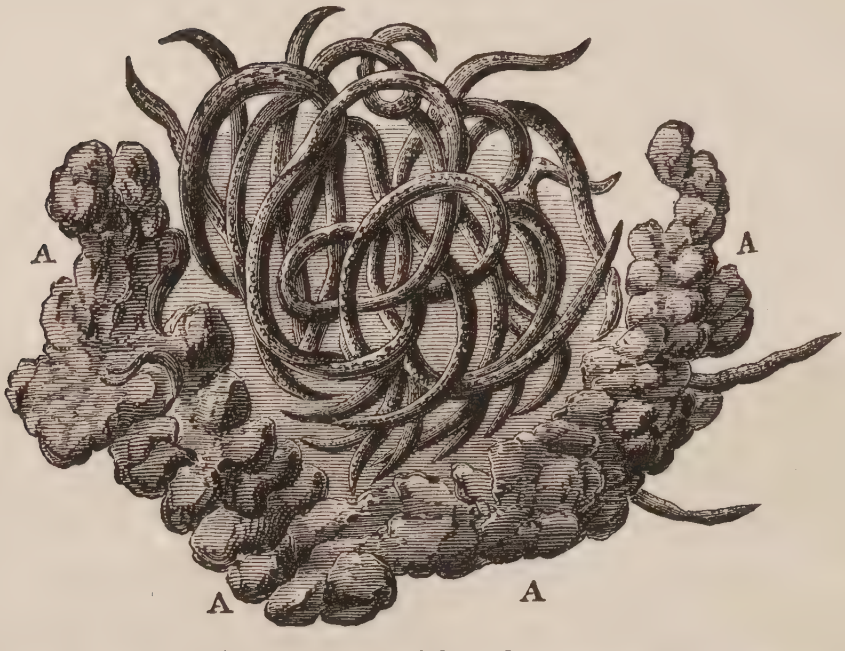
The following shows the oviduct magnified, containing ova; and *a*, *a*, *a*, the young worms, which often escape from the egg when expelled from the oviduct.

Fig. 2.



In the next wood-cut a nest of worms is seen magnified five diameters, surrounded by bony and cretaceous deposit, a degeneration of the granular tubercle-like matter which first appears from the mechanical irritation produced by the female worm and its ova.

Fig. 3.



In young pigs I have met with tubercles in the lungs from a worm very similar to that above described, the *Strongylus paradoxus*. I believe that I was the first to notice these in the pig.

In the wild rabbit in several districts in England the liver is found to be studded with yellow spots, which on a superficial inspection have a great resemblance to tubercles. On a careful examination with the microscope, these yellow spots I found to rise from the presence of ova in the bile ducts, as seen in *Plate* 1, *fig.* 18. I have also depicted the eggs, *fig. g*. As I stated many years since, but few hares and rabbits in England are free from the *Strongylus strygosus* in the first stomach, where they are often found in great abundance. Tapeworm in the rabbit (*Tænia pectinata*) is very common, but to what worm these ova belong I am unable to say. Flukes (*Distoma*) have not unfrequently been found in the liver of the rabbit.

I have also met with tubercle-like spots in the lungs of the polecat (*M. putorius*), the martin-cat (*M. martes*), and the weasel (*M. vulgaris*), from the presence of *strongyli* and *filaria*.

If, as I have said, inoculation with the purulent and cheesy matter of these false tubercles, as shown by Mr. Colin, produces the same effect as the inoculation of the matter of genuine tubercle, it goes far to upset the doctrine of the "specificity" of tubercle.

M. Villemin and others think that there is a great resemblance between glanders and tubercle. I have tabulated nearly every recorded case of glanders in the human subject, and I fail to see the supposed identity, but time does not allow me to enter into this question.

MICROSCOPIC APPEARANCES OF TUBERCLE.

If I were to quote the various opinions respecting the origin

and microscopic appearances of tubercle, from the three zones of Villemin, to the effete and unnucleated character of the cells of some authors, I should fill a great many pages. A good summary will be found in the work before quoted of Hérard and Cornil.

According to my own observations in man and in the lower animals, tubercle begins in the connective tissue of the capillary vessels, as stated by Colberg, Otto, Weber, and others; and is the effect of diseased blood from defective assimilation, and from a peculiar conformation inherited from parents or other ancestors. But although the last named, those having an hereditary taint, are more liable to phthisis, there are probably few children who would not, if placed under certain conditions favourable to its production, (like the lower animals I have described,) fall victims to this disease.

For the microscopic appearances I refer the reader to the first four figures in *Plate* 1, *a*, *b*, *c*, *d*. In tubercular peritonitis (grey tubercle), as seen in *figs.* 2 & 6, I have observed these minute tubercles placed on the small arterial twigs and capillary vessels. In the lungs of monkeys, inflated and dried in the early stage when the nodules are very small, I have also seen them connected with this tissue. In every instance, and I have examined a great many tubercles in the early stage, I have found the vessels in a hyperæmic state, and the parts around in a condition that many would call inflammatory, but probably the capillaries are distended from want of tone rather than from inflammatory obstruction. I have injected the vessels of several lungs with size and coloured watery fluids, but I have never succeeded in injecting the tubercle itself; the vessels run around the edge as in *fig.* 11, but not into it. I need not tire my hearers with a minute account of the microscopic appearances, they may be summed up in a few words: tissue cells occasionally nucleated, running quickly into a state of degeneration, (*fig. d*,) granules, sometimes free nuclei, abortive or imperfectly formed cells and debris, pus-globules, oil-globules, and in the lower animals generally a large amount of cretaceous matter, much of course depending upon the early or advanced stage of the disease.

INOCULATION OF TUBERCULOUS MATTER.

On carefully perusing the account of the inoculation of rabbits with tubercular matter by M. Villemin and other experimentalists, one is obliged to come to the conclusion that tubercular disease in the rabbit and guinea-pig, or a disease nearly allied to it, may be produced by inoculation in many instances. Inoculated rabbits appear to be more susceptible of the disease, although according to M. Villemin it is doubtful whether they are ever affected with tubercle spontaneously, a statement that is easily disproved. Sheep, dogs, and cats when inoculated present doubtful results. But assuming that tubercle may be propagated by inoculation from man

to the rabbit, and from rabbit to rabbit, does it prove the disease to be zymotic? The tubercular granules in the pulmonary, hepatic, splenic, and intestinal capillaries, (smaller in the rabbit than in man,) may act as extraneous bodies and thus produce tubercle, as it is not unfrequently produced in the lower animals, by the ova of worms which occasion a hyperæmic condition followed by caseous and cretaceous deposits. Again, if tubercle can be caused by inoculation is it a necessary consequence that it could be communicated from one person to another?

Before we can arrive at correct conclusions, a large number of substances must be employed that have physically some resemblance to tubercular matter. I hope to undertake a series of experiments that will I think set this matter at rest. It may be found hereafter that other substances besides tubercle may produce nearly similar results.

But there is another matter in connection with tubercular inoculations that it is important to notice, viz., the want of uniformity in the progress of the disease. In M. Villemin's experiments the results were tolerably uniform, but in the experiments of Mr. Simon as published in the Transactions of the Pathological Society for the present year 1867, this want of uniformity is especially noticeable.

Mr. Colin, too, and others are said to have produced tuberculous deposit in rabbits by inoculation with verminous false tubercle in sheep, and the experiments of Cruveilheir and others of the injection of quicksilver into the veins, and the production of tubercle, are well known. The not unfrequent occurrence too of tubercle among certain workmen from extraneous bodies in the air-tubes should not be forgotten. Dr. Marcet, as shown in the present number of the Medico-Chirurgical Transactions, p. 458, has produced tubercle in the guinea-pig by inoculation of the sputa of living subjects, and also from pus and blood of tuberculous patients. As I have said before, the question to be determined hereafter is whether other extraneous matter besides tubercle may not produce the same effect?

From the foregoing I draw the subjoined conclusions.

1. That the evidence we have at the present time does not warrant the assumption that tubercle in man is a contagious or zymotic disease.

2. That the above conclusion is strengthened by the abundant evidence we possess of its hereditary nature, and by the fact that, with the exception of syphilis, no zymotic disease, or tendency to disease, is transmissible from parent to child.

3. That all zymotic diseases observe certain phases of progress, and decline.

4. That ordinary pulmonary tubercle is of uncertain duration, and may often be arrested in its early stage by change of climate and by medicinal and hygienic measures.

5. That tubercular deposit may be produced in man and in the lower animals, (mammals, birds, and reptiles,) by a vitiated atmosphere, bad diet, a change of temperature, and other unnatural conditions.

6. That all deposits or growths of tubercle in man and in the lower animals, (whether miliary or caseous,) are preceded by an inflammatory or hyperæmic condition, and that as far as my examinations have gone, they commence generally in the connective tissue of the air-cells.

7. That tubercular affections in the lower animals (mammals, birds, and reptiles) in confinement are very common, as shown by the numerous preparations and drawings exhibited, and that they differ materially in their nature and seat from those in the human subject.

8. That in the inferior animals the liver and spleen, as well as the lungs, are often affected, whilst, in man, the disease in the two former viscera is comparatively rare.

9. That this deposit in the lower animals, especially in birds, is of a more uniform shape, of a more solid consistence, and contains a large amount of cretaceous matter; that large cavities are but rarely met with in the lungs, and that bloody, purulent, cutaneous, and other exhaustive discharges, are absent.

10. That, as I stated many years since, many specimens of tubercle in the lower animals, as in the examples of rabbits and sheep on the table, have a verminous origin.

11. That I have met with many instances in the inferior animals showing the hereditary nature of tubercle.

12. That the production of tubercle, or something allied to it, in the rabbit by inoculation, requires to be tested by a larger and more varied amount of experiment, before we can arrive at positive conclusions.

13. That, looking to the evidence I have given respecting the supposed origin of some cases of tubercular peritonitis, the effects produced by inoculation,* and especially to the fact, that one form of tubercle, as I believe, in the serpent is contagious, it behoves me to speak somewhat guardedly respecting the non-contagious nature of this disease in man. That the question is one that yet requires much patient labour and research.

For Discussion see page 142.

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* On the 5th of February, 1868, I inoculated a guinea-pig (having previously ascertained that another from the same litter was in a healthy condition) with pus from a whitlow on my own finger. This animal was at first kept in a box, and afterwards had the run of my garden. On the 3rd of March it was found dead, although in good condition. The liver, spleen, and mesenteric glands were tuberculated. Surely without farther prosecuting these somewhat cruel experiments, this is sufficient to show the non-specific nature of tubercle? As I expected, tubercle may be produced by pus and other extraneous substances! See *Plate* 1, *fig.* 15, *e*.

EXPLANATION OF PLATE 1.

FIG.

1. Apex of human lung, showing tubercles in a softened state; (*a*) the same magnified 250 diameters.
2. A portion of peritoneum from a girl affected with tubercular peritonitis; (*b*) the microsopical appearances, 250 diam. See page 118; also Trans. Pathol. Soc. vol. xi. p. 107.
3. A portion of the tubercular spleen of an infant aged three months; Trans. Pathol. Soc. vol. iv. p. 4. The lungs were studded with small tubercles, and the liver was white and fatty; a large number of white corpuscles were present in the blood; (*c*) the microscopical appearance of the tubercle, 250 diam.

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4. One lobe of the thyroid gland of a Wagati Cat tuberculated; the liver and spleen were also tuberculated.
5. A piece of the tuberculated lung of a Spider Monkey (*Ateles ater*) tuberculated; the tubercles unsoftened; (*d*) the microscopic appearances 250 diam.
6. A portion of the peritoneum of a young Monkey affected with tubercular peritonitis; the tubercles are seated upon the small arterial twigs.
7. The tuberculated spleen of one of the Sparrows mentioned at page 122.
8. Tubercles from the liver of a Brown Crane, see page 124.
9. A section of the tuberculated spleen of a Mandarin Duck (*Aix galericulata.*)
10. A section of one of the tubercles of the liver of the Crane, slightly magnified, showing its laminated character.
11. Tubercular deposit from the lung of a Monkey, magnified 10 diameters, showing the vascularity of the surrounding vessels.
12. A section of the inflated and dried tuberculated lung of a Monkey, showing the seat of tubercle; magnified 20 diameters.
13. A portion of the intestine of a Crane, with hard tubercles upon it.
14. Sporules of mould (magnified) upon tuberculous deposit in the lung of a Falcon.
15. A portion of the liver of the inoculated Guinea-pig, mentioned at page 131; (*e*) microscopical appearances, 250 diameters; the tubercle soft, containing a large number of pus globules. (*f*) The blood corpuscles of Brown Crane, page 124.
16. The upper part of the left lung of a Lizard (*U. spinipes*) tuberculated.
17. The small intestine opened of a Serpent (*Boa*); showing tubercular deposit upon the mucous membrane, page 126.
18. A section of a portion of the liver of a Rabbit, showing verminous tubercle; (*g*) the ova magnified.

Drawn on stone from nature, by the author.

AN ESSAY ON SOME DISPUTED POINTS IN THE NATURE AND ANTECEDENTS OF PHTHISIS.

By Charles R. Drysdale, M.D.,
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The author commenced this paper by observing that he had been led to bring the subject of the causes and nature of phthisis again before this learned Association, in order to have the advantage of stating his opinion as to the position of this most important subject at the present moment. During a visit to Paris in the month of August, 1867, he had enjoyed the advantage with many others of having several novel doctrines, such as those of M. Villemin of Paris, discussed and commented on by several physicians, among others M. Hérard. The views entertained by the latter gentleman were so striking, so distinct, and so appealed to his, the author of the paper's, own experiences, that he felt most anxious to hear from others whether they had arrived at similar or opposite conclusions on some of the more disputed points. What, in the first place, are we to understand now by the word Tubercle? Is it the same as granulation, or miliary tubercle? What are the terms, galloping consumption, chronic consumption, to be considered as indicating? The microscope has rendered inestimable service in medicine; are we to thank it for another triumph, and acknowledge that at length it has cleared up the subject of the tubercle corpuscle, or not? In the author's humble opinion it seems that the microscope in the hands of Virchow, Reinhardt, and others, *has* permitted us to establish in an unattackable manner this fact, that the yellowish cheesy masses, considered by Laennec as tuberculous, are only lobular pneumonias, or lobar pneumonias, whose products have undergone a fatty granular degeneration, and that the miliary tubercle alone is a specific character of phthisis.

At any rate, the miliary tubercle is the only lesion which is common to all parts of the body, whether the lungs, the brain, the kidney, or the peritoneum: and it is the inflammations, which its presence gives rise to, which have caused the confusion so observable in the writings of authors upon this subject. The ancient writers confounded phthisis (φθιειν, to dry up) with many other wasting diseases, such as cancer, etc.; and Bayle, in 1810, was

perhaps the first who described miliary tubercle. The immortal Laennec in 1812, described, as all know, the different modifications of the lungs as the evolutions of one tubercular matter. His division of tubercle was into miliary, crude, tubercular granulations, and encysted tubercle. To which he added, unformed, grey, and yellow infiltration. This division of Laennec's has long held supreme sway in the schools, and it was not, perhaps, till 1851, that Reinhardt showed that most of Laennec's specimens of tubercle did not differ microscopically from what is seen in pneumonia, thus partially corroborating the opinion emitted by the illustrious Broussais in 1821, that tubercles were only a peculiar inflammation of the lung.

Cruveilhier, indeed, in 1826, made injections on dogs through the bronchi, and thus obtained little points, at first opaque and puriform, and then yellow, which he believed to be tubercle, as indeed they were, if Laennec's definition were correct. He, then, naturally looked on tubercle as a species of inflammation. In 1844 Lébert described certain tubercle corpuscles as being always found in phthisis, *i.e.*, spherical or rounded corpuscles, without nuclei, ·005 millimeters in size: but Virchow, and those who have followed him, have now shown conclusively that the appearances described as tubercle corpuscles by Lébert, were simply granular nuclei and fragments of epithelial cells undergoing fatty degeneration. Dr. Hughes Bennett, however, still adheres to the views of Lébert, along with many other most distinguished authors.

It appears to the author of this paper, however, now proved, that the masses, called grey infiltration, are only pulmonary alveoli filled with epithelial cells and pus globules, as in catarrhal pneumonia; and that these elements become granular and infiltrated with fatty granulations, as in the termination of all acute pneumonias. Virchow classed miliary granulations among tumours, microscopically characterised by the smallness of their elements and their proneness to pass into fatty degeneration. He contends that these granulations are always seated in connective tissue. They are formed, he believes, by the hypertrophy of the division of the nuclei of the network of cells in the connective tissue. They are the sole specific product of the tubercular diathesis. In the writings of Otto Weber, Niemeyer, 1862, Vogel, 1865, and Frerichs (Diseases of the Liver, 1862), the name of tubercle is reserved for the miliary granulation. Granulations vary from the size of a grain of linseed to that of a scarcely visible point; they are dirty white in colour, at first semi-transparent, but soon cheesy. If a serous membrane containing a tubercle be stretched out on a piece of glass, and magnified 20 diameters, we see a number of white granules generally disposed around little arteries or capillaries, which are congested. With a magnifier of 200 to 500 diameters we find that the elements of the granulations are always the same—nuclei, or small spherical cells ·004 millimeters in

diameter without any nucleolus, packed closely together. In the majority of animals, as the dog, or ape, the tubercle granule conducts itself very much as in man. They are always formed in the connective tissue, wherever they are seated. Cancer granulations are distinguished with some difficulty from tubercular granulations; but their elements are somewhat larger. Syphilitic knots or gummata are not easily distinguished from tubercle in some cases. They are, however, yellow to the naked eye, even when small, and harder and tougher than masses of tubercle. The naked eye appearance of gummata, and their habitual seat in the liver, testis, and periosteum, may help us. Tubercular granulations are most frequent in the pleura, peritoneum, arachnoid, pericardium, and tunica vaginalis, after appearing in other organs. In the mucous membranes they favour the small and large intestines, trachea, bronchi, larynx, and the genito-urinary tract. In the lungs, liver, and kidney they are developed in the connective tissue, and in like manner in the testis. To distinguish granulations from small masses of catarrhal pneumonia, it is to be noted that the first are not so easily crushed; and the microscope shows in the former, nuclei ·004 millimeters in diameter, whilst in the latter there are found cells ·014 millimeters in diameter.*

The lungs are composed of two parts, epithelium and fibro-vascular tissue of the alveolar partitions; and there are three varieties of inflammation of the lungs, lobular, lobar, and interstitial, corresponding to the above-named structures.

Among the majority of those who die of chronic phthisis, we find miliary granulations, lobular, and lobar pneumonias. Tubercular pneumonia plays a more important part in the formation of cavities than granulations do. These cavities are thus naturally connected with a bronchus. With regard to interstitial pneumonia, it may occur in the lungs of the phthisical after hæmorrhage, or repeated congestion, or cardiac affections. It is essentially constituted by a thickening of the walls of the alveoli, which often obliterates them. The tissue "cries" when cut, under the knife. A great deal has been said about the indurated nodosities found in the apices of the aged. These have been considered to be cured tubercular cavities; perhaps with too great facility, since they may arise from old dilated bronchi, syphilitic matter, or simple abscesses of the lung. The amyloid degeneration of the kidney in phthisis is constantly accompanied by a nephritis. The uriniferous tubes are filled with cells, and they are in a state of fatty degeneration.

The combination of the miliary granulation with catarrhal pneumonia, as M. Hérard has admirably shown in his recent work on phthisis, gives rise to three kinds of phthisis. (1) Miliary or

* M. Villemin contends that tubercles in the lungs are formed in the connective tissue, which constitutes part of the alveolar walls, according to him. He considers that crude tubercle is not pneumonia, but true tubercle.

acute phthisis. (2) Miliary granules, limited to the apex, with catarrhal pneumonia, *chronic* phthisis; when more disseminated becoming *galloping*. (3) Catarrhal, or cheesy pneumonia, extending to one or more lobes of a lung, with or without miliary granulations. This last form was called tuberculous infiltration by Laennec.

In the first of these three divisions, the patient may become asphyxiated from the rapidity with which miliary tubercles invade the lung; and the chief symptoms observable will be dyspnœa; 60 to 70 respirations per minute have been observed. Many persons appear to live a long time if the disease does not pass beyond this stage. The typhoid form of acute phthisis has been dwelt upon by authors. One symptom is that of extreme debility. M. Empis has lately written a work in which he contends that this form of phthisis is not tubercular, but the majority of writers do not agree with him in this; and, indeed, to call granulations not tubercle, would be to do away with the word tubercle altogether as a pathological reality, since there is no microscopic distinction between the products of catarrhal pneumonia and phthisis, except in the presence of miliary granulation in the latter.

With regard to the symptoms of the second variety of phthisis—*i.e.*, that wherein the deposit of granulations is limited at first to the apex, and accompanied by catarrhal lobular pneumonia there—the subcrepitating rattle, or rhonchus, so commonly heard at the end of inspiration and expiration, is a sign of congestion and catarrhal pneumonia, and by no means signifies softening of pneumonia. The early stage of chronic phthisis is, by the confession of all experts, in many cases, full of difficulty in the matter of diagnosis. Hæmoptysis, one of the most important signs, is a very fallible one, and my own experience, now considerable in this matter, leads me to endorse the saying of M. Trousseau, that, "If we should add to all the cases of pulmonary hæmorrhage which we observe in hospital practice, those observed in private, we should see that these accidents arise as often from affections which are foreign to phthisis as not." One of the only features of the sputum in any way peculiar to phthisis is the occurrence of elastic fibres in it. The sputum, however, is usually in rounded masses; and in the microscope we see large pavement cells with vibratile cilia and granular corpuscles and pus.

On the head of diagnosis, we have to separate phthisis from chronic pneumonia, which is certainly the disease which most resembles it. The diseases are confounded by many physicians. The chronic pneumonia referred to at present is that which supervenes after breathing for a time some irritating vapour. The vapours which produce it are very numerous. Stonecutters are very subject to it, and it is thus a well-known disease in Edinburgh and Paris. Sheffield grinders die in numbers from chronic pneumonia, and workers in copper are also subject to the same disease.

In the last mentioned case it is the molecules of carbon which play the part of foreign bodies. The lesion of the lungs consists in a thickening of the pulmonary walls of the alveoli, which may obliterate the air-cells, and these latter are often found to be filled with black pigment cells. Such cases are usually quite uncomplicated with tuberculisation. The first symptoms observed are those of great fatigue and feeling of suffocation, with dyspnœa and paroxysms of coughing. Hæmoptysis comes on later in this disease than in phthisis. The respiratory murmur is diminished, and the percussion note becomes dull. Dry or moist subcrepitant rattling is heard; cough with vomiting. The sputum is black in the morning. When cavities form, we may hear cavernous respiration and gurgling. The trade of the patient will assist the diagnosis. Sometimes also the lower lobes of the lung are attacked, which will distinguish the case from one of phthisis. *Dilated bronchi* may be confounded with cavities, but are usually not seated at the apex; and the sputum is fœtid, and not, as in phthisis, in isolated masses.

There is another disease which, in more cases than is supposed by ordinary observers, is confounded with phthisis pulmonalis—viz., *Syphilis*. When, in this disease, the lung is attacked we see symptoms very analogous to those of consumption. The lesion which causes the symptoms referred to is that of softened gummy tumours in the lungs. This gives rise to many of the symptoms observed in advanced consumption, such as cough, expectoration, hæmoptysis, dyspnœa, fever, emaciation, cavernous breathing, gurgling, etc. M. Ricord has dilated, in his lectures, on this interesting subject. "One of the parts of the economy (he says) where gummy tumours are developed more frequently than people would believe, and the knowledge of which is extremely important, is the pulmonary tissue. For some years past we have had a sufficiently large number of autopsies, to make us come to the conclusion, that there are pulmonary lesions which we are forced to attribute to syphilitic tubercle. In the parenchyma of this organ syphilitic tubercle follows the same progress as in any other part of the body. The same form and the same evolution are seen, and the same termination, by melting into pus. The patients spit pus just as in the most advanced stage of pulmonary phthisis. They get thin; weakness and death ensue from the troubles of the respiratory function which supervene. The diagnosis in such cases ought to rest especially on the knowledge of the patient's antecedents. If we hear that he has formerly had hard chancre, followed or not by secondary symptoms; if we find the traces of old ulcers on the tonsils or integuments; if, especially, the chest symptoms are associated with other accidents, in which the syphilitic character is incontestable, such as exostosis, periostitis, etc., we have a right to conclude that the affection of the lung is, like the other manifestations, under the dependence of the syphilitic diathesis. We will add that, even if the practitioner should have some doubt as to the origin of the

old lesions of the economy, or the concomitant ones, he ought to act as if the affections were clearly syphilitic, and administer iodide of potassium, which can do no harm if he is in error, and if he is right will make accidents of the greatest gravity disappear as if by enchantment."

The following case well illustrates the truth of the above remarks. William M., aged 42, was seen by me in the autumn of 1866 at the Metropolitan Free Hospital. The patient had a wan look, and complained of bad throat. On examination I found that the whole of the posterior aspect of the pharynx was occupied by a cicatrix, and the patient informed me that twelve years ago he had had a deep ulceration there, and was under the care of Mr. Coulson for syphilitic disease. He became relieved by treatment, and I did not see him until the summer of 1867, when, on examining his chest, I found all the symptoms of a cavity under the right clavicle. Wishing to give him the chance which has so often succeeded in like cases, I prescribed twenty grains of iodide of potassium thrice a day. The improvement was rapid and most marked. He soon lost many of his worst symptoms, and the sounds under the right clavicle in the course of a month became dry, whilst the patient grew stout and felt well.

Galloping phthisis differs from chronic phthisis only in the rapidity of its course. The lesions found after death resemble those seen in the chronic form of the disease. With respect to that rare form of phthisis, namely, *cheesy lobar pneumonia*, it is in many cases mistaken for pleurisy with effusion; and the diagnosis in these cases resides chiefly in the fact, that in this disease the resonance of the voice is not diminished, as it is in pleurisy. The sputum, too, is not like that of pneumonia, and there is no crepitation heard. The propagation of the heart's sounds is well marked in this tubercular pneumonia, and not in pleurisy with effusion; the expectoration is copious in this disease, and is scanty in pleurisy. Sometimes, it must be confessed, no miliary granulations are discoverable in this form of supposed phthisis. From all that has been said, it appears that phthisis pulmonalis is, above all, a chronic pneumonia of specific nature, and thus, with little difference, we must return to the ideas of Broussais in this matter.

As in the lower animals, hæmoptysis is very rare in infancy, and, contrary to what has been supposed, it appears that acute miliary granular phthisis is by no means such a rare or uncommon termination of life in persons past the age of 70, although this is denied by observers in this country.

ÆTIOLOGY OF PHTHISIS.

Inoculability and Contagion.—One of the most important points in the question, both of the nature and causation of the disease, has recently been opened up afresh by the valuable and startling dis-

covery by M. Villemin, a physician of the Military Hospital of Val-de-Grace, Paris, of the inoculability of granulations upon the lower animals. His first memoir on the subject was read before the Academie de Médecine, December 5th, 1865. His third series of experiments are so conclusive as to be sufficient of themselves to settle the question.

On the 2nd of October, 1865, he procured three pairs of rabbits of about three months old. Each pair were brothers, and of the same size, and each pair was from a different mother.

One rabbit of each pair was inoculated from miliary tubercles, the others were not inoculated; each pair was kept apart in a cage. Inoculations were repeated on October 24th, and on this day a strong adult rabbit was also inoculated. Pair No. 1.—On November 21st the inoculated rabbit was found dead. Small granulations, principally below the pleura. The brother rabbit killed at this time had no tubercles. Pair No. 2.—The inoculated rabbit killed November 29th had a number of miliary granulations below the pleura in both lungs. The brother rabbit was without tubercle. Pair No. 3.—Killed on November 29th. The inoculated one had small grey granulations in the lungs beneath the pleura. Nothing in the brother. No. 4.—The adult rabbit, on being killed, on November 27th, was found to have both lungs covered with subpleural miliary granulations, two of the tubercles of the size of a small pea. On the surface of the spleen the parenchyma was scattered over with granulations.

In the case of rabbits inoculated by MM. Hérard and Cornil, those inoculated with grey granulations had tubercles when killed, and the others had none.

Again, M. Villemin inoculated (1) on December 17th, 1865, a rabbit, which he killed on January 6th, 1866, and the animal already had two budding granulations in one lung about the size of a pin's head. (2) A rabbit, inoculated January 13th, and which had become hectic and on the point of death, was killed on March 16th. It had about the upper part of the left thigh two scabby plates devoid of hair. A little higher towards the gluteal region, there was rather an extensive bare spot with ulceration of the skin. Lungs were full of tubercular masses, formed by agglomerated granulations. Spleen had tubercles, and one kidney contained a large tubercle. Small tubercles in the mesentery and the small intestine. In the case of two guinea-pigs also inoculated at the same time, both were found in a month and a half to have tubercles in the lungs and elsewhere.

These experiments, then, show without doubt that tubercle is inoculable from man on *some* animals.

In 1867 Dr. Marcet experimented on twenty-two guinea-pigs. The expectoration of nine patients with phthisis were inoculated on nine of them. Some of the other guinea-pigs were inoculated with blood, pus, and the sputum of bronchitis. Two were inoculated with sputum from two doubtful cases of phthisis.

Six of the eleven inoculated with the sputum of phthisical patients died, and all had tubercles; one died a few days after being inoculated; and the other four on being killed were all found to be tubercular. The guinea-pig inoculated with the blood of a tubercular human body had tubercles. Two inoculated with empyema became phthisical; none of the rest were found to be so. The sound animals, which were not inoculated, were quite free from tubercles. Blood and pus of the phthisical, as well as their sputum, seem to have the power of causing the formation of tubercles in guinea-pigs, and hence, *probably* in man. I have myself recently repeated these experiments on rabbits at Hampstead, in the North London Consumption Hospital, and have found as yet only a negative result from the inoculation of the sputum of phthisical patients on rabbits. This, of course, in no way invalidates Dr. Marcet's experiments.

These facts establish the specificity of the tubercular diathesis, and although one may not go so far as M. Villemin in the consequences to be drawn from the discovery, and make light of all that has hitherto been written about the causation of phthisis, we must certainly place it, as he does, in the category of virulent diseases, such as glanders, farcy, and syphilis. Nevertheless, the way in which the disease developes itself will not the less continue to be, what it always has been, *i.e.*, spontaneous. Glanders, rabies, and other virulent diseases, may manifest themselves spontaneously, and also by inoculation or contagion; and in phthisis pulmonalis, inoculation, even if possible from man to man, which in all probability it now must be considered to be, will always constitute the exceptional way of obtaining the disease. With regard to the contagion of phthisis, Morgagni, Morton, Hufeland, etc., believed in it, and in Italy and Spain it is an article of medical and popular faith. Henceforth it seems probable that many, both in this country and in France, will become converts to the Italian views on the subject.

Only, as syphilis is inoculable, not contagious, phthisis may be so likewise, but the prolonged cohabitation of husband and wife may in some instances do what a less close and habitual contact would not effect. It has been stated that wives are more liable to die of phthisis after their husbands' death from the disease, than the converse; and if so, we may suppose that the wife may become inoculated by means of the fœtus, as is said to be the case in syphilis. I have myself seen more than one case where married persons have appeared to me to have been affected by contagion from their partners, or at least have become phthisical soon after the death of their partners from this terrible disease.

Hereditary Transmission is certainly, in young persons with phthisis under the age of 30, a most common antecedent of the disease. Perhaps half of all such cases have had a near ascendant phthisical. It is a singular fact that in some parts of South Africa, according to Livingston, the natives are never phthisical. Dr.

Stokes used to say that all men were born with a tendency to consumption, and it is certainly well known that if an animal be shut up in a dark, damp place, with too little to eat, it has tubercles developed, but it would appear that the South Africans do not die of phthisis under any circumstances. I find that phthisis is far more common in town populations than in country districts, and Dr. Edward Smith found that the children of persons with large families were very often affected. This is on account of the wretched diet, so often given to these unfortunate victims of improvident parents. We may be inclined, perhaps, to agree with Mr. Mill, on reading this, that the bringing of a large family into existence in an already over-peopled state like this, is one of the gravest offences against the common weal of which any individual can be guilty. Sad passions tend to phthisis, maniacs and lypemaniacs are liable to die of it. Catarrhs or neglected colds are frequent exciting causes, as also stoppage of menstruation. Pregnancy and child-bearing are both dangerous to a great extent to persons born of phthisical families, and lactation for any prolonged time is especially liable to develope the disease. It does so in cows, according to Dr. Bouchardat, the eminent writer on Hygiene in Paris. It has been said that measles are frequently followed in children by phthisis; but this has not been verified among the better classes.

Tuberculisation ought, the writer believes, most decidedly to be separated from *Scrofula*. Scrofula commences in childhood, tubercles in early maturity. Scrofula has many manifestations, not so phthisis. Then, of 130 children with scrofulous disease of the bones, only three were found by Dr. Coulon in 1861 to be phthisical.

Rheumatism has no connection with tubercles, nor gout either, although we may, it appears, in some rare cases, have a gouty deposit in the lung, which may simulate phthisis. There is no antagonism between herpetism, intermittent fever, or emphysema, and tubercle.

Alcoholism.—Dr. Jackson, U.S., has asserted that phthisis is rare among drunkards. But Dr. B. W. Richardson, Kraus of Liege, and others, among whom the writer may include himself, have collected facts which would show that drunkenness often causes a rapid form of tubercular phthisis, and that in the strongest persons, such as navigators and sailors.

It has been asserted by Drs. Pavy and Wilks, that the phthisis of the diabetic is not accompanied by true tubercular deposit, and certainly miliary granulations are rarely found in such cases. Dr. Bouchardat and others, however, assert the contrary, and believe that this form of phthisis resembles that of those cows which are too long milked and end in becoming tuberculous.

TREATMENT OF PHTHISIS.

It is perhaps unsatisfactory to close any remarks upon the for-

midable disease, Consumption, without a word as to treatment. Up to the present moment, no article of the pharmacopœia has been found powerful enough to put a stop to the specific inflammations seen in this disease. We have not in phthisis, as we have in tertiary syphilis, a drug, like iodide of potassium, which is so well known in the latter disease as a modifier of the specific inflammation which that disease produces in the periosteum and other tissues. That is to say, alas, that we can only endeavour to give our patients as good a chance of life as nutrition of the body will afford; a poor one, as it turns out, in most instances. Good and simple food, in as large quantities as they can digest, with exercise in the purest air we can find for them, are therefore the desiderata to be aimed at in the treatment of the phthisical. That climate is best which admits of their passing the greatest number of hours out of doors; and, so long as they are well muffled up by shawls over the face and wear thick shoes, they should be encouraged to go out even on the coldest days, if they can walk fast enough to keep warm. Such is the treatment which the author is assured will, with the present knowledge of the disease, give the most frequent cures, or at least the best results.

Dr. C. Drysdale begged to say a few words with respect to the important observations made by Dr. Crisp respecting the per-centage of phthisis observed in various parts of the town and country districts of the United Kingdom. Some years ago he had gone into this subject carefully, and had found that the mortality per cent. from phthisis was greatly higher in the towns and town districts, than in rural districts. This fact was especially noticeable in Scotland, where, in some rural districts, the death-rate was very low, and in some, as the North Western islands, phthisis appeared to be all but unknown. The deaths from phthisis, indeed, Dr. Drysdale said, might be taken as an index of the general death-rate of a locality. For example, if Manchester were compared with Westmoreland, it would be found that, just as the longevity of the latter rural district was far above that of the former city, so also were the deaths from phthisis proportionally more frequent in Manchester than in Westmoreland. In fact, phthisis had now taken the place, to a great extent, of the plagues and fevers of former days, and acted the part of one of those sad "checks to population," so admirably described by the illustrious Malthus, and recently by his disciple Mr. Charles Darwin. How much of the mortality of phthisis in towns was to be ascribed to contagion was as yet an unknown quantity, but there could be no doubt that the immense size of our modern cities much favoured the spread of consumption. He ventured humbly, in conclusion, to remon-

strate with Dr. Crisp as to his nomenclature, when he spoke of *tubercles* containing worms. This was a question of definition, and he thought that tubercle meant something apart altogether from worms or parasites.

Mr. Alfred Haviland wished to ask Dr. Crisp if he had, in comparing the relative mortality from phthisis in the different districts of London, taken into consideration the variety in the geological character of the metropolitan area; as it appeared to him to be an important item in the study of the etiology of phthisis. The facts, however, in Dr. Crisp's interesting paper appeared to give a severe blow to the prevalent opinion as to the healthiness of our gravel soil, at least so far as phthisis is concerned, for they prove that this disease is more prevalent in the gravelly districts than in others where either clay or alluvium predominate. The number of deaths at Hampstead, which we all know lies on unmitigated clay, being only 1 in 61 deaths from all causes, and in Poplar, which is built on alluvium, and overlies a submerged forest, 1 in 37; whilst in the gravelly but low level district of St. Olaves, the mortality rises so high as 1 in 11, and even in the gravel district of West London the mortality is so great as 1 in 17. Mr. Haviland thought that the altitude of a district above the river level had more to do with its health than its geological foundation, and he wished particularly to draw attention to the fact that London has an artificia superstratum of granite and granite gravel covering its natural gravel, sand, alluvium, and clay.

Dr. Maund said he was much struck with Dr. Crisp's remarks as to the prevalence of phthisis in some localities and its comparative absence in others. He had lately been making some rather extensive investigations as to the prevalence of phthisis in different parts of the Isle of Wight, and he had found that in that island the disease was localized to an extraordinary extent; for while in some places the cases were exceedingly rare, in others it was almost the natural death of the inhabitants, and yet these localities might be separated by an interval of only a mile or two, and placed under apparently similar circumstances. He would merely instance Sandown. In this district he was much struck with the fact that nearly all the deaths from phthisis occurred in one spot; in the *town* of Sandown certainly not more than two deaths from phthisis, *originating there*, had occurred during the last ten years, in a population of about 2,000; but at Borthwood, a small hamlet in the district of Sandown, with a population of little over 100, fourteen deaths had happened from phthisis in the ten years. Borthwood is in a valley between woods, and the town of Sandown is exposed to the full influence of the sea breeze, and he attributed the difference chiefly to this. The conditions, geological and otherwise, were much the same, and the cases were certified to by the same medical men.

Dr. Wynn Williams remarked that at present he was not inclined to look upon tuberculosis as contagious; that is, that it could be communicated

to a healthy person. But he believed that if a person with any hereditary taint lurking in the system, were placed constantly in close contact with a patient dying of consumption, such as nursing or sleeping with him, and breathing the exhalations from the expectoration, skin, &c., he would be certain to have the disease, then dormant, brought into activity; the anxiety and want of rest of course assisting. He had in this way seen sister after sister carried off, and he made it a rule never to allow one member of the same family to sleep with or nurse another. As regards the statistics of registration, he did not think any reliance could be placed upon them; there was great objection among the lower classes to have the disease registered as consumption, and speaking of Wales, he knew that as it was not necessary to procure a certificate of death from the medical attendant, the disease was quite as likely to be registered wrongly as rightly. Before any reliance can be placed on the Registrar-General's reports of deaths, a law will have to be passed, not permitting any one to be buried without a medical certificate. As regards the habitat of the disease, it appeared to be found where it was least likely, and to be absent from localities where it would be expected to be prevalent.

Dr. Crisp, in answer to Mr. Haviland and Drs. Drysdale and Williams, said that he had not had time to enter into the geological question. As regards the supposed resemblance of glanders to tubercle, as stated by Villemin and other writers, he believed there was a difference in many respects between these affections, as he had stated in his paper. In answer to Dr. Williams, "that the causes of death in Wales were imperfectly registered," he thought that pulmonary consumption was a disease not likely in the great majority of instances to be mistaken.

NOTE ON THE GEOLOGY OF THE PLACES REFERRED TO IN DR. CRISP'S PAPER.

BY ALFRED HAVILAND, F.R.C.S. ENG.

Since the meeting at which Dr. Edwards Crisp's paper was discussed, that gentleman has kindly forwarded to me a copy of the list of towns and counties which he had drawn up in order to show the relative mortality from Phthisis in various parts of England and Wales, and I now append the result of my examination of the relation that obtains between the number of deaths from tubercular disease and the geological character of the places where they have occurred.

In Dr. Crisp's list of 33 county towns I find that really only one half of the geological formations to be found throughout England and Wales are at all represented, and that this small number must be again halved, inasmuch as seven great divisions are only represented by one place each, a defect that would vitiate any comparative analysis. I have not, however,

added to or altered the list in any way, for the special reason, that as it was made without reference to the connection between geology and disease, it affords all the more honest basis to work upon.

In the three great divisions represented I find that the difference in the mortality from phthisis in each is small; for instance, in the towns on the primary or palæozoic rocks, the number of deaths are 1 in 36; on the secondary or mesozoic, 1 in 34; and on the tertiary or cainozoic, 1 in 36. When, however, we compare the subdivisions of these great classes with the mortality, then we find differences, which perhaps, had the list been a more extensive one, would have afforded more complete data for our deductions; even as it is, with the small means in my hands, I think that the following figures show a relation between the geological site of a town or district and the mortality from phthisis, which is well worthy of further investigation, especially in the study of epidemics. The comparative mortality from phthisis, according to the formation of the sites of the different towns are as follows:—

1 in 41	In the	Old Red Sandstone.
1 in 39	,,	London Clay.
1 in 38	,,	Coal Measures.
1 in 36	,,	Chalk.
1 in 34	,,	New Red Sandstone.

These figures are far from conclusive, and teach us little, but they point to a field that is still unexplored, and which when once cultivated, will yield rich results.

We must remember that the old red sandstone sites, which are noticed above as having the least number of deaths from tubercular disease, are to be found in the genial climate of Devon and Cornwall, some of the places being particularly adapted for consumptive persons; the question then arises, is the climate modified by its geological and marine surroundings, or is climate, alone and independent of site, the only cause of the marked difference in the prevalence of phthisis, which is known to obtain throughout our island? Again, in taking into consideration the amount of mortality from phthisis or any other disease in towns, we must never lose sight of the fact that many having their site on such a foundation as the Lower Eocene are necessarily paved, as in the case of London, in such a manner as to alter entirely the geological characteristics of the natural formation. In our metropolis we find a primary rock granite and granite gravel (in macadamised streets) overlying its natural clay and gravel; an artificial state of things which has a powerful effect, not only in modifying the climate of London, but in counteracting many of the evils incidental to its natural site. In studying, therefore, the relation between geology and disease much has to be inquired into, and unless all *τὰ παρέοντα* are duly weighed, much labour will be lost, and much disappointment found.

CASES OF DISEASE OF THE NERVOUS SYSTEM IN PATIENTS THE SUBJECTS OF CONGENITAL SYPHILIS.

By J. Hughlings Jackson, M.D., M.R.C.P.,

Physician to the National Hospital for the Epileptic and Paralyzed, Assistant-Physician to the London Hospital.

It is now widely known, thanks to Mr. Hutchinson, that those who have a particular malformation of the permanent central upper incisors are the subjects of congenital syphilis. Mr. Hutchinson does not say the converse; indeed he has pointed out that it is rare to find teeth so malformed except in the eldest living of a syphilitic family. He has, however, himself recorded exceptions to this rule, and I have published (London Hosp. Rep., 1864, vol. i., p. 384) the cases of two sisters, each of whom had the deformity of the teeth in a well-marked degree. In this family* were several sufferers from nervous affections, including the two who had malformed teeth. This is, however, the only instance in which I have, in my own practice, seen the malformation in two children of one family. Thus my observations tend to confirm, so far as a small experience can be said to confirm a very large one, the observations of Mr. Hutchinson. It is important to keep in mind the fact, that this valuable test of the existence of a syphilitic taint in a family, is to be found usually in but one of the children of that family.

As there may be some who have not observed this particular malformation—it is *very rarely* met with in physicians' practice—

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* There were four children. The eldest, a girl of 18, had good teeth, but remains of old iritis, and scars of ulceration at the angles of the mouth. Her general health seemed good. A girl of 15, whose sight was much impaired from choroiditis, and who had imperfect hemiplegia. This child had the malformation in the teeth. A girl aged 12, who had the same malformation and choroiditis. A boy aged 8, paraplegic, partly idiotic, and who had had several fits. This boy was quite blind; both optic discs were found to be dirty white, margins obscure, vessels small, and fundus hazy.

I will reproduce some woodcuts from Mr. Hutchinson's work, "*Clinical Memoir on certain Diseases of the Eye and Ear, consequent on Congenital Syphilis.*"

Fig. 4.

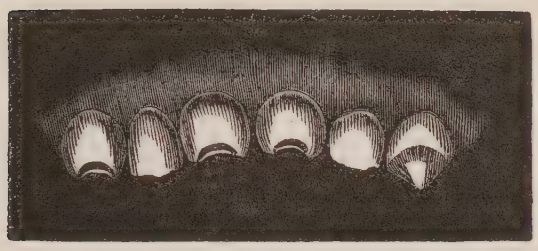

Fig. 5.

It is to be particularly observed, that although Mr. Hutchinson has described many dental peculiarities in children, he relies only—for a test of congenital syphilis—on a *certain* malformation of the two upper central incisors of the *permanent set.* Normally these teeth are chisel shaped, *i.e. broader* at their cutting edges than at their insertions into the gum. The malformation which Mr. Hutchinson has discovered to be a sign of congenital syphilis consists (First) in a reversal of the normal shape so far as this, that the two teeth above named are *narrower* at their cutting edges than at their insertions into the gums. Hence they are then like "screw-drivers." (Second) Further, the teeth are often notched. Hence such teeth are often called notched teeth. It is well to add that Mr. Hutchinson attaches no special importance to "bad teeth," to "irregular teeth," &c., but, I repeat, to a particular kind of malformation of two of the permanent teeth.

In the first of the following cases there is plenty of evidence to show that the patient is deeply syphilised, besides the peculiar shape of the upper central incisors. But in a few cases the only thing to draw attention to the possibility of the existence of the taint is the presence of the dental malformation. Now it is quite true, as I shall afterwards more fully point out, that just such nervous symptoms as the child, Case 1, had, occur in children who present no signs of syphilis. We cannot, therefore, without an autopsy, say with scientific certainty that the symptoms are the result of syphilitic disease of the brain.

Putting on one side the easily explained cases of hemiplegia coming on with valvular disease, or with rheumatic or scarlet fever, most cases in young children of hemiplegia after a convulsion, or cases of convulsion, or epilepsy, cannot be satisfactorily explained. It seems to me that Mr. Hutchinson's work will help us greatly in investigating the nature of these obscure cases. It will help us to determine whether some of them are or are not syphilitic. Now I have, like every one else, made post-mortem examinations of infants

who have died of congenital syphilis, but I have not seen an autopsy on a patient above seven years of age who had died either of congenital syphilis or with symptoms resulting from it. I must then treat my subject clinically, although this method renders us unable to arrive at decided conclusions.

It must be remarked that whilst one child in a family may present in a very striking manner the signs of congenital syphilis, there may be nothing in the appearance of the patient's brothers or sisters to give rise to suspicion. A short time ago this was impressed on me very strongly. In very many cases of epilepsy we find nothing in a patient's bodily condition by which to account, even in the most general way, for the occurrence of fits. We know scarcely more than that there is a person who has occasionally a convulsion. Such a patient was attending at the Hospital for the Epileptic and Paralyzed, when one day her sister came with her. The sister's body presented in an obtrusive manner the signs of congenital syphilis. These were the peculiar malformation of the teeth, scars about her mouth, and nebulous corneæ. Here, then, the suspicion came with force that her sister's fits might be due to inherited taint. I say suspicion, as of course there was not evidence to make it certain that the family constitutional state was really the cause of the diseased nervous system of the one who had fits. Besides the epileptic patient was the elder of the two. The case was such a one as would, I think, generally be called idiopathic or genuine epilepsy. It is all the more desirable when a patient's case appears like the one just mentioned, only as a symptom—a seemingly healthy patient *and* convulsions—that we should extend our line of research in every hopeful direction.

Especially should we follow the example Mr. Hutchinson has set us in studying family histories, and thus we shall get a longer "base line" for the determination of how widely different symptoms arise out of the one degraded bodily state which the offspring of syphilitic parents present. Whilst the eldest child of a family may show external signs of syphilis, such as nebulous corneæ, scars about the mouth, loss of the uvula, etc., the rest of the family may be undamaged externally, and yet be ready to suffer in more obscure ways from their smaller share in their sad common inheritance. It would seem, in acquired syphilis at least, that the symptoms are directly due to a fault beginning in a common tissue, the connective, and the wide distribution of this tissue shows how we may have very different symptoms from its failure in organs or parts of high or low function. When therefore a child is brought to us for an affection so painfully obscure as "genuine" epilepsy, it is well to examine the patient's brothers and sisters for signs of syphilis.

I take as my share of the work to be done the record of cases of congenital syphilis when nervous symptoms are present, although I trust these records will show that I do not take a too narrow view of cases in which congenital syphilis led to nervous symptoms.

Case 1.—*Chorea—Epileptic Hemiplegia—Signs of Congenital Syphilis.*

The patient (Sarah W., aged 10) was admitted September 10th, 1866, under my care, into Charlotte Ward, London Hospital, for hemiplegia of the left side. The paralysis of the arm was quite complete, the leg was so weak that the child could not make an approach to standing, and the face deviated to the right. The orbicularis palpebrarum did not seem to be affected, and so far as could be ascertained from the child, there was at least no considerable impairment of sensation. Her general state seemed to be a comfortable one to herself, and she took her food well. The evidence as to the child's mental condition was chiefly negative. She was dull, taciturn, and easily made to cry.

Now as to the evidences of syphilis. The child's corneæ were nebulous, and it seemed certain that she had had interstitial keratitis, a form of eye disease which Mr. Hutchinson's researches show to be frequently associated with the peculiar malformation of the teeth spoken of, as was the case here. The same evidence proves both to be signs of congenital syphilis. An ophthalmoscopical examination was made, but from the child's inability to keep quiet, and from the partial opacity of the corneæ, no definite results could be obtained. The pupil dilated well with atropine. She had lost nearly the whole of the right ala of the nose and much of the left one, and there were numerous fissures at the angles of her mouth, and many white lines about her face. She had lost the whole of the uvula. The left central incisor was narrowed at its cutting edge, and was notched. The right one had only just appeared. The liver and spleen were not apparently enlarged, and the urine was not albuminous. There was no cardiac murmur, and it may be mentioned that it was afterwards ascertained that the child had never had either rheumatic or scarlet fever.

Subsequent to these investigations I saw the child's mother and then further evidence was obtained, which confirmed the diagnosis of inherited syphilis. The mother had had three children before the birth of the patient. Two of these three died "at the birth," and one came at half its time. Two younger children are reported to be healthy. The patient was at birth (and this is the usual story) a fine baby, and kept in apparent good health for some weeks. At the age of seven weeks "erysipelas" of the face occurred, and carried away part of her nose. At five years of age she had whooping-cough, and at six, bad eyes. The child was then placed under Mr. Dixon's care, and Mr. Dixon—the mother volunteered this—asked "if there had been anything amiss with the (child's) mother or father." So much for the constitutional history. The history of the nervous symptoms is equally interesting.

In the winter of 1864-5—for what precise length of time was not remembered, "it was most of the winter,"—the child had St. Vitus' Dance. The mother is quite positive that the movements affected the left, the now paralysed, side; but she is sure that the side of the face was not worked at all. After getting over this illness the child is reported to have kept in her usual health until August 24th last. On the 23rd she was quite well, and had been out to a school "treat." On the morning of the 24th on getting up she fell and could not stand when raised. She was put to bed again, and quickly afterwards she screamed and went into a fit. She foamed and her mouth was bloody. When the doctor arrived he found that the child had lost the use of her left arm and leg. She did not talk for an hour, but at the end of that time she could talk well, and asked that one of the neighbours might be sent for. At the present time, September 10th, although taciturn, she can talk properly.

Nothing remains to be said of the history than that the iodide of potassium was given in large doses, and that it did her little good. She went out as she came in, paralysed of the left side, but she could walk a little. I saw her several months later, and then she was just in the same state.

It would be mere laziness to conclude that the two symptoms—chorea and epileptic hemiplegia—were due to syphilis merely because signs of syphilis were present, as we know that these nervous symptoms are not usually attended by signs of syphilis. I will speak of the symptoms separately, and first of the chorea.

Chorea in children, generally occurs in those who are healthy. Many of them are delicate, but they are rarely cachectic. They may be tubercular, but they are not often the subjects of actual tuberculization. And whilst they frequently have heart disease from rheumatism they generally have no rheumatic symptoms during the chorea, although sometimes the irregular movements set in in the midst of acute rheumatism, or with pains in the joints. They usually become thinner *after* the commencement. I feel sure that it is an exceedingly rare thing to find decided evidence of the *syphilitic* cachexia in a child who has chorea. When clinical assistant to Mr. Poland at Moorfields, several years ago, I had under my care a girl who had the special form of malformed teeth, and keratitis, and at the same time chorea, and in this case the movements were strictly limited to one side of the body. She got well, but very slowly, both of the keratitis and the chorea, under the use of the iodide of potassium. Yet this case and that of the girl whose case I have given above, are the only ones in which I have met with chorea in patients who themselves presented decided evidence of congenital syphilis. I have however seen chorea in a girl whose elder sister had the malformed teeth, but this child had heart disease, and her family was very rheumatic, *vide infra*. We know neither the seat nor the nature of the tissue changes in chorea. My own

opinion is, that chorea depends on plugging of *small* * branches of the middle cerebral artery supplying the convolutions near the corpus striatum, and that this is often brought about by heart disease, the consequence of rheumatic fever.

Now can syphilis lead to chorea by interfering with the nutrition of these convolutions in an analogous manner?

There being to my knowledge no post-mortem evidence to show how the nervous system is damaged in congenital syphilis, it is fair to see if there are grounds for the supposition, supplied by a consideration of the cases of adults who have died of or with syphilitic disease of the brain. Dr. Bristowe (Path. Soc. Trans., vol. x.), Dr. Wilks (Guy's Hosp. Reports), and myself ("Lancet," Oct. 27th, 1866, and Lond. Hosp. Reports vol. iv.), † have recorded cases in which the large cerebral arteries have been plugged, and it is I think at least possible that the smaller branches may be occluded from

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*When I have suggested embolism as a cause of chorea, it has been objected that the quick recovery of many of the patients negatives such a view. In the first place, patients get well, even from complete hemiplegia, who have undoubtedly suffered embolism. Nay, recovery follows hemiplegia from clot, although a *part* of the motor tract has been *destroyed*. Besides, my speculation as stated in the text, is not that the main trunk or a large branch of the middle cerebral artery is plugged, but that some of the small branches supplying the convolutions near the corpora striata are blocked.

I may refer to the "Lancet," November 26th, 1864, p. 606, where is recorded a case of right hemi-chorea with difficulty of talking. There I adduce as arguments, (besides its occurrence with heart disease, etc.), in favour of embolism, the frequency with which chorea is limited to one side, the fact that it degenerates into actual hemiplegia, and I remark that fright can be the exciting cause only, at all events, in these one-sided cases. Other arguments (London Hosp. Rep. vol. i. p. 459) are that the movements in chorea appear analogous to "ataxy" of articulation, which plainly depends on disease of convolutions near to the corpus striatum. (See also "Medical Times and Gazette," December 14th and 21st, 1867.) The objection that there are very many cases of chorea in which we find no cardiac murmur I have replied to ("Medical Times and Gazette," January 28th, 1865, p. 88) by referring to the statements of Kirkes, Wilks, and Andrew, that vegetations are *invariably* found on the heart's valves in patients who have died of chorea.

The existence of heart disease, or history of rheumatic fever, and the pathological observations last mentioned, justify the belief that there is present a condition for embolism. The frequent one-sidedness, and the degeneration into hemiplegia, point, I think, unmistakably to the region of the corpus striatum. The kind of movements shows, I think, that the disease must be at the very least as high as the corpus striatum. Add to this the greater defect of talking when the movements are of the right side.

One of my patients, a young one—and this case bears, although not closely, on the case in the text—was recovering from chorea, when she was attacked by left hemiplegia. There was a mitral murmur, and no one would hesitate to explain the palsy by embolism, and I think it inferable that the irregular movements had been caused in a similar way.

† See also a most valuable paper by Dr. Moxon, on Visceral Syphilis, Guy's Hosp. Reports, 1867-8. The researches of Reade of Belfast, Todd, Russell of Birmingham, Hutchinson, Wilks, and Bristowe, are now well known.

the same cause. However, I never saw chorea presumably from acquired syphilis, but it is to be remembered that chorea rarely occurs after childhood. The same region must be liable to damage at all ages; embolism may occur at any age, but no doubt the nervous structures and their arterial regions in children are less developed and less educated than in the adult, and thus their deterioration will be followed by different symptoms. But I must grant that the few cases of chorea occurring in people beyond adult age—I have recorded (Brit. Med. Jour. May 18th, 1867) the case of a man aged 74—diminish the value of this inference.

So far then, I think chorea very rarely occurs with evidences of syphilis; frequently with evidences to show that there is a condition (rheumatism or heart disease) under which plugging is admitted to happen. Yet since in adults plugging of the trunk or of large branches of the middle cerebral artery occurs from syphilis, it is, I hold, possible that the smaller branches of this vessel may be occluded as a consequence of congenital syphilis, and that occlusion brought about in this way in young people may lead to changes allowing the irregular movements we call chorea.

Next, with regard to the hemiplegia following a convulsion. As it is well known that such a kind of palsy occurs in the same way in children whose bodies show no signs of syphilis, and whose family histories warrant no suspicion of it, it would be quite unreasonable to come to the conclusion that in the case related syphilis caused the sudden paralysis. It is simply impossible to obtain certainty.

Let us suppose there was a connection. We may fairly take it for granted that the hemiplegia depended on some change in the higher motor tract, probably in the corpus striatum. A more important question than this, is as to the nature of the changes in the corpus striatum on which the hemiplegia directly hangs. To say that congenital syphilis has caused hemiplegia, is to make a statement which even if true is only verbally definite.

If we may follow the evidence afforded by post-mortem examinations on people who have died with disease of the brain the result of acquired syphilis, there are at least three very different ways in which hemiplegia results from syphilis. In not one of the three is nervous tissue primarily at fault. It suffers from the faults of a more vulgar tissue—the connective—and from this at first hand in No. 3 only.

(1) It follows a lump of syphilitic disease of the cerebral hemisphere, distinct from the motor tract, as it follows other sorts of lumps similarly placed.*

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* A lump of anything coarse—a new growth, blood clot, hydatid cyst, etc. —may give rise to changes at a distance from itself. We see part of such changes pretty often by the ophthalmoscope—optic neuritis—and I presume that it is by extension of similar changes to the motor tract—corpus striatum neuritis—that coarse disease in the cerebral hemisphere leads to convulsive

(2) As already stated, it is sometimes the result of blocking of the middle cerebral artery, the coats of which are already affected by syphilis, and then the hemiplegia is analogous to that caused by plugging from heart disease. For in each the change on which the palsy depends is softening of the corpus striatum.

(3) It may be the result of a syphilitic nodule which has grown in the motor tract itself as other sorts of nodules do.

Now it is perfectly plain that in each of the three instances there is a different thing for treatment, and thus that the term syphilitic hemiplegia is really most vague; and we see how it is when we come close to the positive tissue changes, that in undoubted cases of syphilitic diseases of the nervous system we quickly cure some of our patients and do no good to others. We cure those patients who come to us for *recent* palsy of cranial nerves; or, in other words, we can easily procure the absorption of *recently* effused lymph, whether it be in a nerve bundle, or in the iris. But in the several sorts of hemiplegia just mentioned, to get rid of the palsy we have in the 1st, to reverse changes diffused from a foreign body; in the 2nd, to treat softening of nerve tissue, the result of cutting off the blood supply by a mechanical obstacle; and in the 3rd only have we to treat actual syphilitic disease which is then squeezing nerve tissue. So then to speak of treating syphilitic paralysis is to speak with mere *verbal* definiteness.

On which of the three changes did the child's palsy depend?

(1) Hemiplegia from a lump of disease in the cerebral hemisphere usually comes on by convulsion, as was the case here. But the hemiplegia so resulting is usually passing, and sooner or later, either before or after, there is severe pain in the head. Optic neuritis is a frequent complication, but its presence could not be established here. In the vast majority of cases there would be further convulsions.

(2) The growth of a nodule in the motor tract is unlikely. Hemiplegia might, it is true, come on suddenly from such disease, or there might be no palsy at all. But I think it most improbable that it would cause a severe convulsion followed by hemiplegia.

(3) I incline to the diagnosis of plugging of the middle cerebral artery or of some large branch of it, although I admit that in adults plugging of this vessel rarely causes convulsion. In acquired syphilis the artery becomes nodose, and its channel may be narrowed, and then blocking easily occurs.

So my hypothesis is that each of the symptoms in this case, (the irregular movements and the temporary convulsion followed by the permanent hemiplegia,) were dependent on blocking—the chorea on that of small branches, and the epileptic hemiplegia on that of the

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seizures and to epileptic hemiplegia. I must here refer for further remarks on this subject to papers I have published. Roy. Lond. Ophth. Hosp. Reports, vol. iv. part 4; vol. v. part 1; vol. v. part 4; and for a brief summary to the "British Medical Journal," March 28th, 1868.

trunk—of the middle cerebral artery. The girl recovered from the chorea, I imagine, because the lesion was slight; she did not recover from the hemiplegia, because a larger vessel being plugged a large quantity of nerve tissue was partially destroyed.

I next relate several cases in a family in which there was clearly a rheumatic tendency, but also to my thinking a decided syphilitic taint. Here, however, the evidence of syphilis rests solely on the malformation of the teeth Mr. Hutchinson has described. Having seen many cases in Mr. Hutchinson's practice in which this malformation was evidently the result of syphilis, (see the evidence in his book), I have for my part no doubt at all that there was a taint in this family. To say of the cases that they were epileptic, etc., would satisfy nobody. The problem is, how did it come to pass that the family suffered so much from nervous symptoms. Martha had fits of a kind for which we usually discover no positive cause. To put her case on one side as one of genuine or idiopathic epilepsy would be useless. I think it probable that one of her cerebral hemispheres, or perhaps both, had been damaged by syphilis. To inquire in the direction of syphilis and rheumatism is at all events to work in a realistic manner. I have fewer doubts as to the case of Mary, on account of the amaurosis (optic atrophy). This symptom in the vast majority of cases in young people points to organic disease within the head. Julia owed her chorea I imagined to blocking of small branches of the vessels supplying convolutions near the corpus striatum, and the mitral disease, no doubt the result of rheumatism, countenances this opinion. Syphilis most likely had nothing to do with her illness.

Cases 2, 3, *&* 4.—*History of Rheumatism; Syphilis?—Epilepsy—Amaurosis—Chorea.*

In 1863 Mrs. K., about fifty years of age, brought her eldest daughter to me for epilepsy at the Hospital for Epilepsy and Paralysis, and at the same time consulted me about a younger daughter who passed large quantities of muco-purulent urine. The younger child had well marked syphilitic teeth, and very recently has had epileptiform attacks and amaurosis.

The mother, who is now healthy looking, had chorea when she was a child, and she remarked "it attacked one side only." She had it a second time when pregnant with her eldest child, but only for a week. Of course it is doubtful whether the second attack was chorea or not, yet she was confident that it was like the first. She had been pregnant thirteen times, four stillbirths and nine born living. Of the nine two died, so that seven are living.

[Subsequently to this note Mrs. K. had rheumatic fever; afterwards I found valvular disease, and she died of the effects of this lesion about a year later.]

The eldest living, 20 years of age, is quite healthy, but he had two fits when at school, which however his mother says were fainting fits only. It is to be feared, however, they were really epileptic, as it will be seen that his sister Martha had the so-called faints first. Julia has "faints," followed recently by amaurosis. I have little doubt but that he will have genuine fits in time. [I afterwards attended him for an attack of rheumatic fever.]

The next, Martha, aged 16, is the patient who has severe epilepsy. She walked and talked early, but at the age of three months she had a swelling of the right elbow, then in the other joints, then in the back. She was ill three weeks and was then well, and kept well generally until she had the fits. About a year before the fits she had measles, followed by whooping-cough, but has had no cough nor any evident tuberculous symptoms since. When ten years old she began to have attacks, in which at first she was simply giddy, and her mother said "would almost fall;" sometimes, when standing up she would shake all over. Up to this time she had been quick and intelligent, but now study was interdicted, and she was sent into the country. A month later, in spite of this care, she had her first severe attack of convulsion. At first she had a fit once a month, and gradually oftener.

Careful inquiries were made as to "convulsions" in infancy, teething, worms, injury, etc., but there was nothing positive found. She had never menstruated. She was thin, pale, and languid; and her mind was defective. She would sit for hours and take no notice, especially *after* the fits. She would then sometimes cut up the sheets or stand naked, but *before* the attacks her mind was said to be clear and she behaved properly. Her temper was rather perverse, but she was never violent. Masturbation had been suspected, and she had been very carefully watched, but it was never detected. Her teeth were well formed. She was convulsed on both sides, and she had no "aura" from a limb. She had the sensation at the epigastrium so common in what is called idiopathic epilepsy, but she had it for some hours before the attacks. They never lasted more than five minutes. She had the fits about twice a week when she came under my care. Bromide of potassium did her some good, but there was no decided permanent benefit.

Her sister Mary, aged 13, began five years ago to pass a large quantity of thick mucous urine. Then there was also an occasional streak of blood. When I saw her she was thin and wan, and was still passing the urine described, yet she had no pain, and, except the urine and the emaciation, had no decided ailment. The urine was milky-looking, and contained flocculent matter which settled to the bottom. By heat it was made more milky, and by nitric acid it became more minutely flocculent.

Her teeth were malformed as in congenital syphilis, (Mr. Hutchinson was kind enough to look at them for me,) but she had no opacity of the corneæ, and had never had any inflammation of the eyes. Her

sight was then supposed to be good, but unfortunately I did not test it.

She improved remarkably under quinine and iron, but could not take cod-liver oil. About the end of July she began to have what her mother called faints. When out she would suddenly fall without obvious cause, occasionally the sight of the eyes failing first. She sometimes had several of these attacks in a day, and sometimes passed a week without one. On September 18th, however, her mother brought her to me on account of defect of sight. The right pupil was smaller than the left, and she could only just see shadows with the right eye. She said that this eye had been weak for some time, but that it had decidedly failed only a fortnight before. Practically she was blind on the right side, and on the other the sight was very much impaired. Both optic discs were atrophied, but the right much more so than the left. (Unfortunately I have no better account of the ophthalmoscopical appearances, so that it is not plain whether the optic atrophy was simple or after optic neuritis.)

In November she began to be deaf on the left side, but this has not progressed.

In February, 1864, she began to menstruate, but still did not improve as regards the faints, the amaurosis, the deafness, nor recently in the condition of the urine. Yet she looked better in general health.

May 10th, 1864. A few days ago a new symptom appeared. One day when walking the right foot twisted spontaneously, the heel being turned a little out and the toe in, and the foot placed so that the part touching the ground in standing would be the outer border. This only lasted a short time, but she had then pain in the calf, and could not for a short time straighten the leg. She had now pain over the left temple. I feared that this was a further development of epilepsy, but nothing more came of it.

The next patient of this family was Julia, aged 9. She was brought for slight deafness on the right side, attended by some discharge. This soon passed off, but in January, 1864, at the time when her brother had rheumatic fever, she had chorea. At first the movements were on both sides, but soon the left only was affected. It continued so for about four months. She is now, May 11th, quite well in every respect, except that she has a mitral murmur, as she had when the chorea began. I regret that I did not examine the heart when she had the deafness only. She is a delicate-looking, blue-eyed pretty child, and has a great deal of light silky hair. There was nothing about her to make one suspect syphilis. [She subsequently died of heart disease.]

The next case I relate is interesting because both mother and son had the dental malformation so often mentioned. He, however, does not suffer in any way. His sister has fits, but presents no signs to warrant the diagnosis of syphilis. The condition of her mother

and brother however renders it likely that she owes her diseased nervous system to transmitted disease.

Cases 5 & 6.—Signs of Congenital Syphilis in Mother and Son.—Epilepsy in the Mother and in a Daughter.

Mary W., aged 28, came under my care October 22nd, 1866. A month before, when in a shop, she had a fit which came on whilst she was "laughing and talking." She became insensible, was carried home, was put to bed, and knew nothing of her state until three hours after, when she came to herself. She had had nine fits altogether, and in the last three the attacks had been preceded by a curious sensation in the head and by some kind of movement of the nose. She had had much violent pain in the head betwixt the fits. She had nebulous corneæ, her eyes having been bad when she was 18 or 19. She was then blind three months, being unable to see anything. Now the patient was married *before* her eyes were bad, and her first baby was nine months old when they were affected. It may be supposed then, that these symptoms were the result of acquired disease, especially as we found nodes on her tibiæ dating three years back, and scars of ulcerations in the same place. But so far as I know, acquired syphilis does not produce keratitis, which she had evidently had, and it certainly could not be the cause of the dental malformation, which was well marked. It was clear to my mind that she was the subject of inherited syphilis, but she may also have acquired disease as well. Mr. Hutchinson, who saw her and her family, with me, was of this opinion. It seems the more likely from the fact that her eldest boy has also the dental malformation. It is the only suspicious sign he has that I can find, or that I can hear of, signifying inherited taint. Now this woman's third child, five years old, has fits, and has been subject to them for three years. The child has no warning, and I know nothing more of them than that they are attacks of convulsions. She seems healthy.

I took the mother into the hospital for a short time, and she went out better in general health.

December 9th, 1867. I called on her and found her suffering much from a recent node on the left side of her head, at the occipito-parietal junction. She was suckling a healthy-looking baby three months old. Except that she rapidly improved under the use of iodide of potassium I know no more of her case.

Case 7.—Epileptic Hemiplegia, with Congenital Syphilis.

The seizures, so far as one can tell by the rather vague description which the mother gave, resemble those we not unfrequently see as one result of acquired syphilis. Whenever, in an adult, a fit begins by cramp in one hand or in one foot, or in the side of the face, and

especially if the patient is hemiplegic afterwards, we ought to inquire carefully for syphilis. I have not seen, except in the following instance, fits of that kind in well marked congenital syphilis. Mr. Hutchinson has recorded several in his work above referred to.

The patient whose case is next related I saw in the practice of Dr. Brown-Séquard at the Hospital for the Epileptic and Paralysed. I have recorded it in the "Med. Times and Gaz.," June 22nd, 1861.

Edward R., aged 14. June, 1861. He was quite well until he was three months old; he then had a rash all over him, "sores and boils," for which he was under medical care nine months; he did not use any ointment. He recovered to some extent, but was always delicate. He became able to walk at two years of age, and talked very early. At the age of four he was paralysed, and ever since he has been subject to fits. Whilst out playing at the age above-mentioned, he was seized with "a fit" which his mother said lasted from eight in the evening till two next morning; no doubt there was a succession of fits. It was found afterwards that he was paralysed on the right side. For three weeks he did not speak, but he soon recovered the use of the right side, although not to walk. His mother says that she had again to teach him to walk. He slowly and gradually recovered speech and power of motion. He had another fit six months afterwards. They gradually increased in frequency, and he now has them every week. When the fit is coming on he gives a scream, *the right arm and leg are drawn up*, he then becomes insensible and is convulsed. Until very lately the convulsive movements were entirely confined to the right side, but at present the other side also is affected. His mother says that in the fit his mouth is drawn to the left. The duration of the fit varies; it is generally five or ten minutes, but it has been as long as three and a half hours. He generally sleeps for some hours after the fits. Three years ago his eyes were bad, and now both corneæ show the remains of keratitis, but there is no iritis. His upper central incisors present the malformation spoken of. His mind is evidently very weak, his manner restless and feeble, and his memory, especially for events, very bad. He cannot read, which may be accounted for somewhat by his imperfect sight, and also by the want of proper trials to educate him. His mother does not seem to suspect any taint, and no direct questions were asked.

In the same place, and in the next volume of the "Medical Times and Gazette," are recorded other cases from the practice of Mr. Hutchinson.

Case 8.—*Convulsive Seizures in a Girl who has Malformed Teeth—Nervous Symptoms in the Child's Father.*

This case is interesting, as showing in another way the value of Mr. Hutchinson's researches. As will be seen, I take for granted

in this instance that the peculiarly malformed teeth are by themselves sufficient evidence to warrant the diagnosis of congenital syphilis. It is only necessary to give the case in the merest outline. I speak first of the girl's father, for it is as furnishing evidence of the nature of his case that the child's state is of importance for my present purpose.

J. M., aged 45, was admitted into the London Hospital on October 2nd, 1866, and was by the courtesy of Dr. Andrew Clark transferred to my care on the 16th. He was lying in bed, apparently suffering intense pain in the head, and nothing could be got from him except vague complaints of this pain. His speech was not affected, although he would talk but little, saying chiefly, "Oh, my head." There was no evidence to show that his sight was affected, but it was impracticable to test it, except in the roughest manner. It was easily ascertained by the ophthalmoscope that there was double optic neuritis.

Now the only history which could be obtained of this illness was, that a week before admission he had a fit in the street, and although he walked home after it, he was confused, and had been, to use his wife's expression, "in a stupefied state ever since." He had had severe pains in his head, but no vomiting except for a few minutes on one day. His only previous illness consisted in "rheumatics," which were possibly pains of a specific nature.

Under the use of iodides and bromides he recovered with great rapidity; went out apparently well except for damaged optic nerves. A patient who has optic neuritis is never safe, and he soon after came to the out-patients' room for more convulsions.

So far in the history, here is the case of a man who has had convulsions, severe pain in the head, and double optic neuritis. Whilst these symptoms declare conclusively, not only that there is intracranial disease, but that that disease is of a "coarse" kind, (see footnote, page 152,) they tell us nothing as to its particular nature. The coarse disease might have been "tumour," "abscess," etc. The patient denied having ever had syphilis.

In July 1867, his daughter, 12 years of age, was brought to me for convulsive seizures, to which she had then been subject eight weeks. She had no warning, fell suddenly, was convulsed, did not bite her tongue. Now this girl had narrowed upper central incisors, and they were slightly notched. Mr. Hutchinson was kind enough to look at them, and declared them to be characteristic. She had nebulous corneæ, but this was doubtfully the result of past interstitial keratitis. Then the child's nose was sunken. The mother had had no miscarriages; the patient was the only child living. One child was dead born, and one died in half an hour.

In this child's case treatment by both iodides and bromides has been unsatisfactory. I grant that the knowledge that the child was the subject of syphilis did not help me to cure her. It is, I feel con-

vinced, not warrantable to infer that "syphilitic epilepsy," either in the adult or in the child, should be easily curable by iodides, as there is no evidence to show that the changes in nerve tissue, on which the fits *directly* depend, are syphilitic. (See remarks on the three pathological varieties of syphilitic hemiplegia, page 153; also foot-note page 152; and a note on Substitution Nutrition, Roy. Lond. Ophth. Hosp. Report, vol. v., part 4.)

The child's state, however, threw light on her father's case. It seemed to me almost certain that the lump of coarse disease inside his head—which the severe pain in the head, convulsions, and optic neuritis, declared to exist—was syphilitic "deposit," which had its origin in pia-matritis, if I may coin a word analogous to iritis.

The further history of the father's case confirmed this view. He next, October, 1867, had a recurrence of the severe headache, with partial deafness of the left ear and incomplete facial paralysis of the same side. Next, November, 1867, palsy of the left third nerve, and on February 18th, 1868, he was admitted under my care for hemiplegia of the right side and total loss of speech. I know of nothing but syphilis which produces such a disorderly *succession* of symptoms at distinct intervals over so long a period.

[Since the above was in type the patient has died (on March 27th, 1868). The autopsy showed syphilitic disease of his brain, and thus confirmed the diagnosis founded on the above-mentioned clinical evidence.]

Excepting convulsions I have seen but one case of marked nervous symptoms in an infant who at the time showed signs of syphilis. In the exceptional case which I saw in Mr. Hutchinson's practice, there was spasm of the muscles supplied by the portio-dura nerve and paraplegia. I have, with the exception of this case, not yet seen affection of any motor cranial nerve with signs warranting the diagnosis of congenital syphilis. This is the more remarkable as palsy of the cranial nerves not unfrequently occurs from acquired syphilis. I have seen, in children, palsy of the third nerve, apparently causeless, and in some of these cases syphilis may have been at the bottom of the mischief.

I was consulted the other day by a woman 35 years of age, who had had palsy of both third nerves, and of both sixth nerves, from the age of six years, after scarlet fever. Were it not that syphilis rarely produces such symmetrical and complete local nervous symptoms, I should imagine there was inherited taint in this case. Except for suspicious looking scars about the mouth, attributed to smallpox when three years old, and for the fact that her mother had been twelve times pregnant and had but three children living, there was nothing to countenance the supposition.

I have seen cases of deafness from congenital syphilis, but chiefly in Mr. Hutchinson's practice, and for an account of these cases I may refer the reader to his book above-mentioned.

CASE OF DIPHTHERIA SAVED BY TRACHEOTOMY: WITH REMARKS ON THE OPERATION.

By George Buchanan, A.M., M.D.,

Surgeon to the Glasgow Royal Infirmary, Lecturer on Anatomy, &c.

I desire to draw the attention of my fellow-graduates to the surgical treatment of Diphtheria in its later stages. In numerous papers on this subject, among which I may refer to one contributed to the proceedings of the British Medical Association at Cambridge, and another in the "British Medical Journal" for the 2nd March, 1867, I have stated that I do not propose tracheotomy as a cure for all cases of diphtheria; but so much misconception prevails on this subject, that I may be pardoned for reiterating my views. It has often been objected, that as diphtheria proceeds from a blood poison, tracheotomy is inadmissible, since it only deals with one of the symptoms, leaving the primary disease unchecked. To this I answer that in many cases within my own experience the operation has warded off impending death from suffocation, and afforded time for the patient to throw off the disease, with the result of a permanent cure.

Although diphtheria occurs with great severity in epidemic seasons, the Registrar-General's returns show that it prevails as a constant disease in these islands, and that the general mortality among children depends in an appreciable degree on its ravages. Now it is matter of common observation that it proves fatal in one of two ways, according to the type of the disease, viz., by asthenia or by suffocation. A close examination of the symptoms from a comparatively early period of the attack will indicate in what direction the fatal issue is to be feared, and what I urge is that, in the suffocative type, recourse should be had to the surgical operation before the patient is worn out by the deficient aëration of the blood. In the asthenic type, when the tendency is to death by the virulence of the poison, no one would perform tracheotomy even though some dyspnœa should occur; but in the sthenic form, when suffocation is impending, surely it is our duty to try to save life whatever be the primary malady. The propositions may be shortly stated as follows:—

1st.—Diphtheria proves fatal, in a notable proportion of cases, by suffocation, caused by the exudation extending down into the larynx.

2nd.—Of these fatal cases a fair proportion might be saved by the operation of tracheotomy.

3rd.—There is a stage in the disease, sufficiently well marked, at which the practitioner can affirm that his remedies will prove of no avail, and that fatal suffocation will ensue.

4th.—At that stage immediate recourse should be had to the operation, experience proving that it is very successful when resorted to comparatively early.

My experience of croup and diphtheria, although limited, is very remarkable. During the last seven years I have been called to upwards of forty cases. I have not seen any cases of either disease at the outset; having been called upon in my surgical capacity by the family practitioners to determine the question of tracheotomy and perform it if decided on. In thirty-one cases I performed tracheotomy, and of these, eleven recovered perfectly. The cases I refused to operate on were *in articulo mortis*, or were in a stage when there was apparent hope of recovery by perseverance in medical treatment. But what is remarkable is, that none recovered except the eleven which were saved by tracheotomy; and it is this consideration which has determined me to undertake the operation in future even earlier in the disease than I have previously ventured. I would strongly urge this view on my medical brethren, in the hope that the mortality from the suffocative form of diphtheria may be lessened by at least one-third.

With regard to the operation, I have nothing to add to what is described in works on practical surgery—except to express my decided disapproval of all instruments designed to induce the operator to make a rapid plunge into the trachea, instead of dissecting carefully down to the point at which it is to be opened. I am satisfied that the contrivers of such stabbing instruments can have had no experience in operating on children. The patient should be put under the influence of chloroform; the incision should extend from the isthmus of the thyroid gland to near the sternum; the tissues should be divided layer after layer, till the white rings of the trachea are seen bare at the bottom of the wound. A sharp hook is then to be fixed in the upper part of the trachea to steady it, while a sharp knife is pushed rapidly into the tube, the back of the knife being upwards and the wound extended downwards as far as necessary to admit the silver tube. A pair of closed dressing forceps is to be put into the incision, and the blades held apart, till all the exudation is coughed or pulled out; in a few minutes the struggle for breath will be quieted, and the silver tube can be readily lodged in the trachea. These proceedings demand the utmost patience and calmness on the part of the operator; the motions of the larynx caused by the irregular attempts at inspiration require the greatest caution

to be exercised in the application of the knife during the stage of dissection, and all unnecessary hurry is almost fatal to the successful completion of the operation. Lastly, constant attention to the tube by a skilled attendant for the first forty-eight hours is imperative.

The following short notes of a successful case will illustrate the principal points alluded to:—

On the evening of the 27th October, 1867, I was called by Dr. Miller to visit a child suffering from diphtheria. The boy, aged five years, had been ill for two or three days, but only the day previous had Dr. Miller been called to attend him. He found the child presenting all the symptoms of diphtheria and prescribed accordingly. On the morning of the 27th the symptoms had become aggravated, and breathing was performed with difficulty. Dr. Miller, feeling sure that the disease was progressing rapidly, called for my assistance.

I found the little patient in bed, tossing about in his struggles for breath, then lying down exhausted by the effort. After watching him for a few minutes, I became convinced that there was no hope of his recovery unless the respiration could be speedily relieved. Inspiration was performed with a loud crowing noise, and there was a rough barking cough. The ensiform cartilage and intercostal spaces were violently drawn in during each effort to obtain breath, showing that obstruction to the air-passage was considerable. The tonsils were covered with the white diphtheric exudation, so that it was evident that the false membrane had extended from the mouth into the larynx. I stated my opinion that the child had no chance of recovery except what the operation afforded. After some hesitation the parents requested me to undertake it. The little patient was put under the influence of chloroform, which is a great help to the operator, and the operation successfully accomplished as above advised. The patient was laid near the fireplace, and a kettle of water was kept boiling on the fire, the steam from the spout of which was allowed to moisten the air around the child's bed. This I think a most useful precaution. As is usual in such cases, the little patient had fallen into a quiet sleep before I left the house, having been worn out with want of rest for twenty-four hours previously.

The progress of the case was most satisfactory. The tube was taken out on the seventh day, and the child made a rapid recovery. Exactly three weeks after the operation the child was brought to my house, when I found the wound quite healed, and the boy restored to health and strength.

Dr. Davey, (Northwoods,)—I am very glad to have the opportunity of speaking to this question of tracheotomy in children, because some three or four years since I expressed an opinion not a little favourable in regard to the use of Dr. Marshall Hall's tracheotome, which an additional experience goes far to contradict. A case of croup in which I opened the trachea with the tracheotome is recorded in the pages of the Journal of the "British Medical Association." To this case I was called in consultation with Messrs. Salmon and Long of Thornbury. The symptoms were urgent, and the ordinary treatment having failed we decided on *tracheotomy*. Chloroform was administered, though very carefully, and the folded or closed blades of the instrument were passed into the windpipe. Before the separation of the two sides of the *tracheotome* was accomplished, (and not a moment of time was lost,) the little patient had died. At this time it is evident to me that the trachea, already so seriously obstructed by a morbid secretion, became literally blocked up with the cutting part of the instrument, and so suffocation directly ensued. The death of the child, it was *then* agreed among us, was due to the *chloroform*. However, it was not very long after that I saw, with Mr. Crossman of Hambrook, another case of diphtheritic croup. It was agreed to open the trachea. The operation was performed, not now by myself, and this time without *chloroform*. The same tracheotome was employed by Mr. Crossman, and with precisely the same result, viz., immediate suffocation on the passage of the closed blades into the obstructed trachea. The deaths of these two children were precisely alike in all particulars. I agree with the author of the paper that the tracheotome must be proscribed.

Dr. Crisp observed that the great difficulty was to know *when* to operate in croup; he had performed tracheotomy five times in this disease. In two cases the benefit was very marked, but the patients died from obstruction to the tube. If he had to operate again he should be induced to snip off portions of the tracheal rings. Unfortunately, in genuine croup the mischief was not confined to the larynx and trachea, but often extended to the bronchial tubes. The great success of this operation in the hands of Trousseau was well known, but then it was supposed that many of his patients were not affected with genuine croup.

Dr. Wynn Williams, whilst agreeing with the author on the advisability of an early operation, hoped he might be permitted to call the attention of the members to a paper on the subject under discussion just about to be published in the Obstetrical Transactions, wherein he strongly recommends

the application of a strong solution of tannic acid to the diphtheritic membrane, which regularly tans it, and causes moreover its immediate expulsion. Of course, the difficulty is to apply it where the disease has extended down the larynx. In any future case he would endeavour to apply it by means of Dr. Richardson's ether spray producer. He believed it to be of the utmost importance to get rid of the offensive membrane, as many of the patients die of septicæmia caused by the absorption into the system of the putrid emanations from it.

Dr. MAUND said, in his experience of diphtheria, which was tolerably extensive and amounted to nearly 300 cases, he never remembered to have seen more than one case decidedly fatal by suffocation, the fatal result occurred from asthenia or from blood poisoning, and he would be inclined to consider many of Dr. Buchanan's cases as cases of croup. Possibly the epidemics of diphtheria presented different symptoms in different localities. Dr. Maund's cases occurred chiefly in East Kent, between the years 1856 and 1862.

The PRESIDENT, that the Members might rely with confidence on the statements of his friend Dr. Buchanan, whose accuracy of diagnosis was well known. He mentioned this in Dr. Buchanan's absence in reply to Dr. Maund's suggestion.

FISTULA IN ANO HEALED BY LIGATURE.

By Draper Mackinder, M.D., F.R.C.S.E., &c.

"One of the most disagreeable companions a man can take about with him is a *fistula in ano*, a painfully suggestive associate that is constantly reminding the sufferer of the very probable existence of an occult and formidable underminer of his health. Even though this remorseless antagonist to comfortable locomotion and repose, instead of being the mere representative of a less conquerable foe, bore with it the tangible evidence of independent responsibility, the consciousness of having to undergo a painful and somewhat dangerous operation, which, in many cases, necessitates the recumbent position for weeks, is of itself sufficient to create anxiety in the strongest mind.

"A few weeks' incapacity for the ordinary duties of business-life would, to some people, be a matter of the most serious importance, involving a pecuniary sacrifice amounting almost to ruin. To a needy professional man a prolonged bedroom residence would be peculiarly unfortunate; to the Esculapian most depressing, seeing that it affected materially the welfare of more than the individual sufferer. Any effectual means, therefore, that can be adopted for the avoidance of the series of ill-consequences alluded to, instead of being allowed to fall into desuetude, should be hailed as a boon to suffering humanity."

Such were my sentiments in 1859, when, in the "British Medical Journal," I published some cases of fistula in ano treated by ligature. Since that time I have had under my care many cases more or less severe, which I have treated by ligature with invariable success.

In the paper alluded to, I described my method of operating, but to meet certain difficulties which I have since encountered, I have had some other small instruments manufactured for me by Mr. Wood of York. My little case now contains the following articles:—Two flexible silver probes seven inches long, one blunt and the other sharp pointed at one end, and both perforated at the other; two tubular steel directors five inches long, slightly bulbed at one end, and flattened and heart-shaped at the other, the bores being of different sizes; one large common silver probe, perforated at one end; one small short steel probe with a flattened annular extremity; one pair of forceps; three sets of ligatures made of silk and hemp, and armed with gut or bristle; some strong silk cord; a coil of fine silver wire; and a box of white wax. Thus fortified, the most troublesome case may be managed with ease.

METHOD OF OPERATING.

Having by previously administering an aperient prepared my patient for the exploration, I introduce the blunt-pointed probe, armed with silk, into the external opening of the fistula, pass the index-finger of the left hand into the rectum, search for the opening, seize the extremity of the probe, which is easily bent, draw it through the anus, and complete the operation before the sufferer is made aware of the nature of his ailment. In the event of there being no internal opening, or that aperture not being easily found, I pass the steel director along the sinus, then through it with the sharp-pointed probe transfix the wall of the gut, the probe being, as in the former case, armed with the ligature. When the fistula runs a long way up, say from three to four inches, the silver probe cannot be bent by the finger in the gut without giving a good deal of pain. To obviate this, I pass either the large silver probe or the small steel one into the bowel along the finger, until the eye of the probe rests on the tip of the finger, and then through that eye I push the end of the long probe and bend it towards the anus. This being done without strain on the bowel, gives very little pain. But in these formidable cases, when there is an internal opening, I now dispense with the long silver probe and simply pass a bristled ligature through the tubular director into the gut, and draw it out with the finger or forceps and tie it. This manipulation gives no pain worth mentioning, and is quickly and easily effected.

One of the tubular directors may be used, if preferred, instead of the stiff probes, for passing up the gut on the index finger, the notch at the top having been so shaped as to allow the flexible probe to be bent over it.

The ligatures are of four kinds. (1) Thick silk for the flexible probes; (2) thick silk with bristle fixed at one end; (3) strong platted silk with gut at one end; (4) strong hemp with gut. Sometimes I make a fifth by twisting a little silver wire with one of the others; this is occasionally necessary when, in consequence of the large substance to be deligated, softening of the silk or hemp takes place, and a fresh ligature has to be introduced. I have found the silk more durable than the hemp. Sometimes I pass a thick ligature, sometimes a thin one, but never tie it tightly, since by so doing much discomfort is caused for many hours and without manifest benefit. Unless the ligature come away before the whole of the intervening part has been divided, nothing more is required to be done, and the patient may move about and live as he likes. From three weeks to three months will elapse before a cure is established.

REMARKS.

When we bear in mind that this plan of operating is bloodless, comparatively painless, without danger, creates no alarm, necessitates no dieting, and neither requires absolute rest nor indoor

residence; when we think of all the attendant drawbacks of the operation by the knife, the fear, the mental shock to patient and friends, the pain, the protracted suffering, the danger in some cases, the long confinement to hospital or bed, the tedious convalescence, the ruinous expense; when we bear in mind these advantages and these disadvantages, and further recollect that there are cases in which no other than the operation by ligature could be entertained—an operation too, let us remember, ever happy in its results—I think I shall be justified in asserting that the common operation with the bistoury is no longer advisable, no longer to be encouraged, but, on the contrary, ought henceforth to be regarded, what it i in cutting fact, *a fundamental error.*

The following letter is from one of my patients, a solicitor, and describes the only case I think it necessary to record here in support of the practice I have been advocating.

" Gainsborough,

" My dear Sir, " May 22nd, 1867.

" I am sorry I have been so long in writing you my promised letter respecting the fistula and piles which I had last autumn. You operated upon me by putting in the first ligature, consisting of a kind of twine, on the 12th of September last, I being then nearly 51 years of age and nearly 17 stones weight. The ligature came out on the 25th of September, and on the 26th you put in another, which also came out on the 26th of October, on which day you put in a wire ligature. My case appeared to create considerable interest as to the mode of your treatment, and I was induced when in London, to consult Dr. Curling, who is, I believe, an author of a work on this subject, and an eminent practitioner. I saw him on the 25th of November last and was carefully examined by him, when he recommended me to be cut, which I refused to do. I was subsequently induced to consult an eminent surgeon who has known me for many years, and after examining me he concurred with Dr. Curling. I was again urged to obtain further advice, and being again in London I consulted Sir William Fergusson, Bart., on the 13th of December last, and after examining me he pronounced *in almost your own words* that I should soon be cured, and upon the 4th of January last the wire ligature came away imperceptibly, since which time I have felt no indication of a return of the malady.

" I beg you again to understand most distinctly that I never once lost my confidence in your skill and treatment, and only consulted the gentlemen above-named at the request of my friends. I was able with a few trifling exceptions to attend to my business during the whole period named.

" You can make any use you like of this letter, and with ma ny thanks for your careful consideration of my case,

" Believe me, yours very truly,

" Draper Mackinder, Esq., M.D. " F. Wood "

This was one of the best cases I have met with for testing the patient's confidence, the surgeon's tact, and the tenacity and utility of the ligature. The internal opening was about four inches up the gut, the external two inches to the right of the anus, and the base of the isoceles comprehended a bunch of angry piles. Heart's action feeble, adipose tissue abundant, demands of business heavy, ill-advisers numerous. The distance of the internal opening made the bending of the long probe difficult, and the strain on the gut disagreeable, hence the suggested stiff probe and the bristled or gutted ligature. The separation of the hemp and of the silk after a month's exposure, induced me to strengthen the llgature with a coil of silver wire. The recommendation of Mr. Curling and other chirurgeons to substitute the knife for the ligature, when but a small point of this once extensive district remained to be *fairly* divided, and the subsequent endorsement by Sir William Fergusson of the operator's opinion, tell their own moral. In spite of adverse authority and antagonistic desires my patient recovered and remains well.

A CASE OF GASTROTOMY FOR EXTRA-UTERINE PREGNANCY.

WITH A TABLE OF THIRTY-TWO CASES OF RECORDED GASTROTOMY.

By D. Lloyd Roberts, M.D., M.R.C.S.L.,
Surgeon to St. Mary's Hospital, Manchester.

Eliza S., æt. 30, was admitted an in-patient of St. Mary's Hospital on November 24th, 1858. She had been married, and previous to her admission into the hospital had been under the care of Mr. Brady of Tunstall, Staffordshire, who kindly gave me the following account of her previous history. He was engaged to attend her in her second confinement, her first child being seven years old. Her former labour had been natural, but she had a bad getting up, having had leeches applied, and been bled from the arm three times in succession. Mr. Brady further informed me that when he was called to her he found her suffering from what he considered spurious pains, and on examination was sure that there was something unusual about the case, in fact he was satisfied that a tumour which he discovered was external to the uterus, but was not sure that it was a child. He had the opinion of the late Mr. ——, who agreed that it was a fit case for operation. Another medical gentleman who also saw the case thought that it was a malignant tumour unfit for operation; several other medical opinions were obtained, and she ultimately came to Manchester. The following particulars are from the patient herself. She had menstruated regularly, and had commenced at about 14; but she had not menstruated for eighteen months previous to her admission. She stated that she considered herself in the family way, as she experienced all the usual feelings which attend pregnancy, and that at the end of what she supposed to be the ninth month of utero-gestation, she had had all the symptoms of labour; the abdomen had enlarged from month to month up to the end of nine months. She said that she had frequently felt the child. An examination of the abdomen showed it to be enlarged. On the right, extending upwards beyond the umbilicus, downwards towards the iliac fossa, and laterally a little beyond the median line, was an irregular ovoid tumour, exceedingly sensitive on pressure or manipulation. On examining the most prominent part of the tumour by palpation, a decided crepitation, pro-

duced by the overlapping of the cranial bones of a fœtus, could be distinctly made out. On examination per vaginam the os uteri appeared a little higher and more to the right than normal. It was small, and admitted the uterine sound to the extent of three inches; the os and cervix uteri were normal. The uterus was not so mobile as natural. On the right side of the pelvic cavity a hard resisting tumour could be felt. The patient appeared to be getting worse daily, apparently from the absorption of purulent matter, although no part of the tumour appeared as if it would break or point; the integuments covering it were of an uniform colour throughout; there was no fluctuation. She had hectic, night sweats, vomiting, inability to take nutriment, and a small, weak, and frequent pulse. A consultation being held, the case was decided to be one of extra-uterine pregnancy, and gastrotomy was determined upon. The dangers of the operation were explained to the patient, and she consented rather than suffer any longer. Accordingly on January 20th, 1859, she was placed on the operating table. Chloroform being administered by Mr. Runcorn, house surgeon to the hospital, I commenced the operation by making an incision four or five inches in length over the most prominent part of the tumour until the cyst was exposed. This being opened a fœtus, compressed, doubled upon itself, and bathed in several ounces of dark puriform offensive matter, was found to occupy the cavity. The child, a male, was at once removed. It measured 17 inches in length and weighed 4½lbs. It had undergone decomposition in several parts, particularly the scalp, the cranial bones grating freely over one another. The right forearm was also much decomposed, the elbow joint was broken and completely disorganised. The skin peeled off readily, and appeared to be converted into a fatty substance.

There was no vestige of the placenta. During the opening of the cyst no hæmorrhage occurred; the cavity was carefully sponged, and the external wound united by sutures and plaisters. A pad and roller completed the dressing, and the patient was put to bed. Four hours afterwards her pulse was 130, and in the evening 140; an opiate was given at bedtime, and she had some little sleep during the night. In the morning of January 21st she appeared to be sinking, and notwithstanding the administration of diffusible stimuli and nutriments she died about 12 a.m.

Post-mortem examination. On removing the ligatures and gently separating the wound it was found that no union had taken place. Although there was some little effusion of lymph there was no fluid of any consequence in the peritoneal cavity. The peritoneum presented the appearance of old as well as of more recent inflammation. Bands of lymph connected the omentum, mesentery, and small and large intestines, to the cyst and to each other. The only portion of peritoneal surface free from signs of inflammation was that which covered the superior and anterior portion of the

uterus. The uterus was drawn upwards from its central position and to the right; it was somewhat larger than a healthy multiparous organ in the unimpregnated state. The os uteri was in a perfectly healthy condition. The left Fallopian tube and ovary were normal, with the exception of being bound by bands of false membrane to the back of the uterus; the Fallopian tube was nearly throughout impervious; the ovary was flattened and shrivelled. The walls of the cyst were formed exclusively by the expansion of the right Fallopian tube. The parietes of this cyst, varying in thickness from a quarter to half an inch, had contracted firm adhesions below to the pelvic fascia, vessels, and nerves, externally to the caput coli, ascending, and transverse colon, and to the ilium two inches above the ileo-cœcal valve. At this part the sac itself was firmly bound to the ilium. It was also *firmly* bound to the ascending colon. It occupied a transverse position in the pelvis, its length being about eight inches, its breadth seven inches. The interior of the sac was rough, not unlike that of the deciduous lining of the uterus. There existed no mark by which the site of the placenta could be ascertained. There was no communication between the cyst and the cavity of the uterus. A fine probe could be passed into the right Fallopian tube for about an inch. There was no trace of the right ovary.

I have brought this case before the Association not solely on account of its rarity, but because it has suggested to me to collect in a tabulated form as far as I could all cases of extra-uterine pregnancy wherein the operation of primary gastrotomy has been performed. This tabulation indeed has already to some extent been effected by the labours of Mr. Jonathan Hutchinson, but I have been able to increase to twice its original extent the list published by him in the "Medical Times and Gazette" of July 1860. These cases fully testify to the advisability of performing the operation in all conditions of the system where the powers of life are fast sinking, and in which nature is making no effort to get rid of the fœtus either by suppuration externally or through an opening into the vagina or bowel. I have not thought it desirable to include in my table cases of what Mr. Hutchinson very appropriately calls secondary abdominal section, where ulceration has already been established, and in which the operator merely enlarges a breach previously commenced by nature. It will be seen that these cases constitute a class differing in many important points from those of primary gastrotomy, and are much more favourable in their termination, for, as Mr. Hutchinson says when comparing these two classes of cases, "in the one in all probability a healthy peritoneal cavity is laid open, while in the other adhesions having formed, the incision amounts to little more than opening an abscess. In the one the decomposing fragments of the placenta, the fœtid pus, etc., will have access to the peritoneal sac; while in the other they are shut off from all chance of effusion into it. In the one a healthy woman is subjected to all the danger

of a sudden and most severe operation, for which her system has been in no wise prepared; in the other a source of irritation is removed from the organism at the very time when it was becoming felt to a degree almost incompatible with the continuance of life."

Out of the thirty-two cases tabulated I find that fifteen terminated favourably to the mother, being nearly fifty per cent. In three cases the children were saved. In one instance only did both mother and child survive, and this remarkable case acquires additional interest from the fact that the placenta was spontaneously expelled. The attempts that have been made with the view of saving both mother and child by gastrotomy have been signally unsuccessful. Hence authorities, from the time of Campbell (to whose valuable memoir on Extra-Uterine Gestation I am indebted for the first twelve cases in my table) downwards, are opposed to the removal of the fœtus until the excitement inseparable from gestation has subsided, and the system of the parent has been restored to its unimpregnated condition or nearly so.

Case 20 deserves a passing remark from its being one of twins. In the preceding pregnancy delivery had been effected by the Cæsarean section, and the rupture in the uterus was discovered to be coincident with the line of incision which had been made during the Cæsarean operation.

In my own operation, as mentioned in the report, not a vestige of the placenta remained. But where it is present I should feel disposed to advise its extraction, if this can be accomplished without much difficulty, in order that one great source of irritation should be removed.

I wish also to make one remark on the question which was raised by Mr. Spencer Wells in November 1860, in a discussion on Mr. Adams's paper on Extra-Uterine Fœtation at the Royal Medical and Chirurgical Society as to the propriety of the removal of as much of the cyst as can be safely separated. In my own case this proceeding would have been almost impossible, from its numerous and firm adhesions to the viscera, pelvic fascia, and nerves. If, however, another case were to present itself to me where the cyst was not very adherent and the adhesions could be easily separated, I should be disposed to remove as much of it as I could without too great disturbance of neighbouring parts, for the reason already given for the detachment of the placenta, viz., because it would remove a great source of irritation after the operation. On the whole, a careful consideration of the recorded cases irresistibly points to the conclusion that gastrotomy may fairly rank among the legitimate operations of surgery, the proportion of successful cases being nearly fifty per cent. It may be remarked that the recoveries, in Great Britain at least, after the Cæsarean section, amount to no more than twenty per cent.

No. of Case.	Date.	Age.	OPERATOR.	No. Pregnancy.	Time of Death of Child to Operation.	Result.		Where Reported.	REMARKS.
						Mother.	Child.		
1.	1760	28	John Bard	2	Many months	L.	D.	Med. Obs. and Inq. Lond., vol. ii. p. 369, 1760.	A fœtus extracted; much pus evacuated. No mention is made of the placenta.
2.	....	..	Mr. Clarke	..	....	D.	D.	Mem. Med. Soc. Lond. vol. iii. p. 197, 1792.	Pains continued eight days; dead fœtus and placenta removed. Mother died, four hours afterwards, from loss of blood and exhaustion, produced by operation and removal of placenta.
3.	1791	..	Wm. Baynham	3	10 years	..	D.	Med. Facts and Obs., vol. i. p. 73, 1791.	Putrid fœtus extracted in pieces. No placenta found.
4.	1789	36	Marchi.	..	.. .	D.	D.	Comment. de Rebus, vol. xxxv. p. 289, 1793.	Fœtus extracted, which had been retained three months beyond the usual term. Death thirty-eight days after the operation. Placenta probably removed, but not stated.
5.	....	..	Dr. McKnight	..	13 mos.	L.	D.	Mem. Med. Soc. Lond., vol. iv. p. 342, 1795.	Gastrotomy performed, left side. No placenta found.
6.	....	..	.. ..	..	3 years.	L.	D.	Ed. Jour. Med. Sci., No. 3, p. 223, 1827.	Child removed partly putrefied. Patient conceived and bore a child whilst pregnant with extra-uterine fœtus.
7.	....	30	.. ..	..	Not stated	D.	D.	Jour. de Med. Chir. Phar., vol. xxii. p. 437, 1811.	Putrid fœtus removed. Woman died in two days; cyst, on post-mortem examination, was found on right side.
8.	Feb., 1814	38	.. ..	5	....	D.	L.	Nouv. Jour. de Med. Chir. et Pharm., vol. xv. p. 51.	A well-developed female child removed alive. Woman died of a slow fever. Post-mortem examination. Placenta adherent to fundus uteri, ovary, and tube.
9.	April, 1819	21	.. ..	..	3 days	D.	D.	*Ibid.*, vol. x. p. 500.	A putrid fœtus extracted; left tube involved. Patient died eighteen days afterwards.
10.	....	..	.. ..	..	....	D.	D.	Paul B. Calvo, Hist. de l'Acad. Roy. des Sci., 1714, p. 29.	Fœtus in right tube (nine months) extracted in a state of decomposition. Patient died eleven days after operation.

No. of Case.	Date.	Age.	OPERATOR.	No. Pregnancy.	Time of Death of Child to Operation.	Result.		Where Reported.	REMARKS.
						Mother.	Child.		
11.	....	22	.. ..	..	7 weeks	D.	D.	Bononiensi Institut, 1767, Obs. 1532, p. 363.	Left tube was ascertained to have constituted the cyst of the fœtus.
12	October, 1823	38	.. ..	..	....	D.	D.	Philadel. Jour. Med. Phys. Sci., vol. i., 1825.	The placenta and a portion of the membranes found in the right tube.
13.	Sept. 23, 1835	28	Francis Hutchinson	1	3 or 4 months	L.	D.	Med. Gaz. vol. xvii., 1835.	Child removed; funis divided; placenta left. Sat up and walked about on October 26th, 1835.
14.	Aug. 27, 1836	38	Mathieu	4	6 or 7 months	L.	D.	Med. Chir. Rev., 1842, vol. xiii. p. 545, & Annal de la Chir., Sept., 1841.	A child, 19 inches long, was removed. Placenta removed. Much pus discharged during convalescence.
15.	1837	..	Schreyer	3	....	L.	L.	Schmidt, Jahrbücher, Band cx., 1861, from Monatschr für Geburts-kunde, vol. xiv. p. 283, Oct. 1859.	Placenta spontaneously evolved.
16.	....	..	.. ..	..	....	D.	D.	Schmidt, Jahrbücher, vol. xxxix., 1843, and Med. Annalen, bd. viii. heft 3.	Placenta in part removed.
17.	May 22, 1850	30	Prof. Johnson	..	About 9 months	D.	D.	Phil. Med. Ex. N.S., vol. vi. pp. 511-522; & Med. Chir. Rev., 1851, vol. vii. p. 273.	A full-sized fœtus, weighing 9 lbs., was removed, and also the placenta, which was adherent. She did well for sixteen days, and died of diarrhœa.
18.	1852	34	Genth	4	....	L.	(?)	Med. Chir. Rev., 1867, vol. xix. p. 278, & Verhandl. der Ges. für Geburtskunde Berlin, 1855.	A fœtus, four or five months old, was removed, which lived for a moment after extraction. Greater part of placenta removed, with much loss of blood.

No. of Case.	Date.	Age.	OPERATOR.	No. Pregnancy.	Time of Death of Child to Operation.	Result.		Where Reported.	REMARKS.
						Mother.	Child.		
19.	Dec. 6, 1852	..	Rousseau	2	About 3 months	L.	D.	*Ibid.* 1855, vol. xvi. p.558: and Bul. Gén. de Thér. Mai, 1855.	Fœtus removed; placenta and membranes left.
20.	Nov. 5, 1852	..	Decoene	2	Not quite a day	L.	D.	Schmidt, Jahrbücher, vol. lxxviii., 1853, from Gaz. des Hôp. 1852, p. 147.	Twins. The preceding delivery was effected, by Decoene, by the Cæsarean section. The uterus was seen to be well contracted. There was to be seen, along its entire anterior surface, a rupture, corresponding to the line of incision made at the time the Cæsarean section was performed.
21.	Jan. 20, 1859	30	Dr. Lloyd Roberts	2	9 months	D.	D.	St. Andrews Graduates' Transactions, 1868.	A decomposed child removed; no placenta; no hæmorrhage. Cyst left. Great exhaustion before operation.
22.	July 24, 1859	18	Dr. Da Silvi Lima	1	Several months	D.	D.	Brit. Med. Journ. May 12, 1860, p. 360, from Gazeta Medica di Lisboa, March 1st, 1860.	The fœtus removed; placenta intimately adherent, removed by fingers with difficulty. Wound of the cyst brought together by serres-fines, except the lower part. Weight of fœtus $8\frac{1}{2}$ lbs.
23.	Oct. 24, 1859	43	Dr. Goodbreak	10	2 years and 9 months	D.	D.	Med. Chir. Rev., 1860, vol. xxvi. p. 553. and Bost. Med. Surg. Jour., July, 1860.	Fœtus removed with difficulty, owing to adhesions between it and sac. Placenta and part of sac removed; no blood lost. Died on the fifth day.
24.	Aug 21, 1859	40	Dr. Stutter	5	1 month after full time	L.	D.	Med. Times and Gaz. July 21, 1860.	Placenta adherent to intestines; no attempt made to get it away. No cyst. Child (a female) removed; weight $5\frac{1}{2}$ lbs.
25.	May 31, 1860	28	Dr. Ramsbotham & Mr. Adams	1	6 months	L.	D.	Med. Times and Gaz. July 21, 1860; & Med. Chir. Trans., vol. xxvi. p. 1, 1861.	Placenta not removed, as it was attached. She recovered well by July 18th, 1860.

No. of Case.	Date.	Age.	OPERATOR.	No. Pregnancy.	Time of Death of Child to Operation.	Result.		Where Reported.	REMARKS.
						Mother.	Child.		
26.	Dec. 1860	32	Rektorzik	6	....	D.	L.	Schmidt, Jahrbücher, Band cx., 1861, from Oesterr. Zeitschrift f. prakt. Heilkunde, vol. i.-viii., 1860.	Placenta not removed; no hæmorrhage. The mother died the same day.
27.	April 2, 1862	32	Dr. B. Hicks	2	Many months	D.	D.	Guy's Hosp. Rep. vol. viii. N. S., 1862.	Putrid fœtus removed; no trace of placenta or funis. Died twelve hours after operation. Cyst not adherent to abdominal parietes. There had been a communication between cyst and intestines.
28.	May 15, 1862	40	Dr. B. Hicks	..	Some years	L.	D.	Guy's Hosp. Rep. vol. viii. N. S., 1862.	Fœtal bones removed; cyst adherent to abdominal parietes. Cystitis in consequence of a communication between cyst and bladder.
29.	1862	..	Dr. Pauk	..	....	L.	D.	Allg. Wien. Med. Zeitung; & Med. Chir. Rev. 1862, vol. xxix. p. 554.	When six months advanced in pregnancy had diarrhœa. Patient recovered in a fortnight.
30.	Apr. 21. 1864	40	Dr. Greenhalgh	2	Child extracted alive about 8th month.	D.	Died in a few minutes.	Medical Mirror. vol. i. p. 689. 1864.	Mother in a dying state at the time of operation. Fœtus much distorted. The difficulty of diagnosis in this case was due to the position of the fœtus, which lay with its abdomen much stretched against the abdomen of the mother; the limbs, head, and chest, being doubled backwards against the spine, so that the heart could not be heard, and no limbs could be felt.
31.	1865	30	Dr. G. Philippart	1	10 mos.	L.	D.	Gaz. des Hôpitaux, 1865; & Med. Chir. Rev. 1866, vol. xxxvii. p. 535.	Child's head closely adherent to the cyst. Impossible to extract the whole fœtus, but several bones were brought away; subsequently all were removed.
32.	1865	..	Baeza	9	8 months	L.	D.	Schmidt, Jahrbücher, 1866. Band cxxix.; & Siglo Medico, Mayo, 1865.	Some bones appear to have passed *per vaginam;* but the aperture seems to have quite healed before gastrotomy was performed.

DESCRIPTION OF PLATES 2 & 3.

PLATE.

2. Fœtus after its removal from the Cyst.

3. Uterus with Cyst; showing the opening made for the removal of the Fœtus.

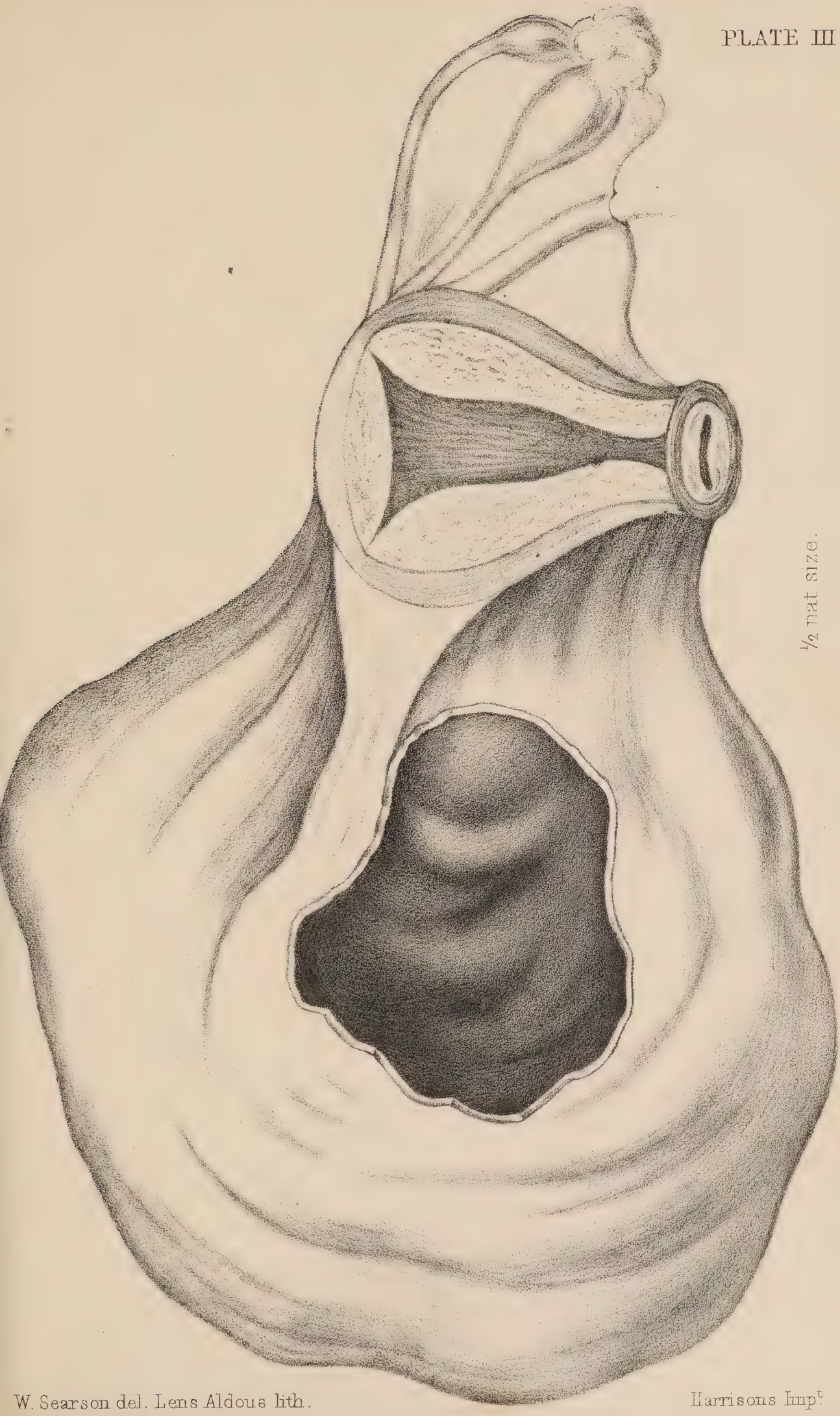
PLATE II.

½ nat size.

W. Searson del. Lens Aldous lith.

Harrisons Imp^t

PLATE III.



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CASE OF PLACENTA PRÆVIA.

RAPID EXPULSION OF PLACENTA AND MEMBRANES ENTIRE.

By Joseph Smith, M.D., M.R.C.S.,
Physician to Warrington Dispensary.

Ellen M., of Warrington, aged 37 years; has been married eighteen years, and has had ten children and two miscarriages. She has always had rapid deliveries, her medical attendant never having been present at the birth of the child, except in one instance. She is a spare, middle-sized, dark-complexioned woman, of very active habits.

On the evening of Wednesday, July 10th, 1867, she requested my attendance. She had been seized with profuse uterine hæmorrhage, and sent for me in great alarm. On inquiry, I ascertained that she was in the eighth month of utero-gestation. The hæmorrhage had ceased when I arrived, and did not recur till the day but one following (Friday). I then made an examination. There was no dilatation of the os uteri, nor were there any uterine pains. Slight discharge continued during Friday night and the whole of Saturday, and as she seemed restless and excitable, I gave her a full opiate at bed-time. I saw her again on Sunday, at a quarter-past twelve at noon. She had passed a quiet night; had had scarcely any pain, and very little discharge. She appeared to be in good spirits, and expressed herself as feeling much better. At twenty minutes to one I was hastily summoned, and was at her bedside in three minutes. She was lying on her back, and on examination I found in the bed, and between her thighs, the placenta and membranes entire, the placenta being rendered convex by the pressure of the child's head against it. I at once ruptured the membranes, and the child began to cry.

Mrs. M. told me that soon after I had left her at a quarter-past twelve she had been suddenly seized (during the absence of the nurse from the room) with a violent pain in the belly that continued for two or three minutes. By one strong expulsive effort the uterus completely emptied itself, the placenta, the membranes, and their contents being discharged in one entire mass.

My patient recovered without a single bad symptom, and mother and child are now quite well.

This case is interesting—

First—On account of its satisfactory termination, mother and child both being saved.

Secondly—Because of the rapidity of the process. It did not occupy more than two or three minutes.

Thirdly—In its medico-legal aspect.

Dr. Davey mentioned that more than thirty years since he had met with a similar case, in which the whole ovum with the membranes intact passed rapidly during a strong uterine contraction through the os externum; having rapidly torn open the membranes he tied the funis, and mother and child did well.

NOTE ON ERGOT OF GRASSES.

By R. Uvedale West, M.D., F.R.C.S. Edin.

A very intelligent friend of mine, Mr. T. C. Johnson, of Tothby House, near Alford, who occupies a large farm in the parish, some weeks ago showed me some specimens of what he thought must be ergot on some Italian rye-grass. He thought that the sheep and other cattle which had grazed on this grass had cast their young prematurely in an undue proportion, and he was anxious to know whether, if the specimens shown to me were ergot, these too-frequent cattle abortions might be caused by the animals having eaten some of it.

The question is undoubtedly one of great interest, and the more so, when, on my telling Mr. Johnson that the disease on the rye-grass was undoubtedly a very fine specimen of ergot, and a very likely thing to cause abortion in sheep and cows which were in the habit of eating it, he made a further search in his pastures and found many other natural grasses infected with the disease.

I lay before the Association specimens of various grasses more or less infected with ergot.

Postscript.—When I mentioned this subject in casual conversation with another large farmer, he caught eagerly at the idea, saying that probably ergotism would explain a fact which had caused him much thought. A valuable brood mare which had already bred two or three foals, being during early utero-gestation put to graze on rye-grass, aborted two years in succession. Thinking that in some way the herbage did not suit, he ever after grazed the mare on old pasture; the result being a lively foal every year while he continued to breed from her. The fact given above would undoubtedly be more convincing if my friend had noticed the *presence* of ergot on his rye-grass. Nevertheless I append his statement—*valeat quantum*. Ergot is certainly more common on rye-grass than on other grasses, and likely to be more potent.

Dr. Crisp said he did not think that the ergot had much to do with this disease, for ergotted grasses were so common, that if such affections were produced by them they would be much more frequently met with.

THE ELECTRICITY OF THE BLOOD CONSIDERED IN ITS RELATION TO THE PROCESSES OF DIGESTION AND ABSORPTION OF CHYLE.

THE TESTIMONY AFFORDED BY THE SPECTRUM ANALYSIS AS TO THE CAUSE OF THE RED COLOUR OF THE BLOOD, AND A REVIEW OF THE ELECTRIC CHARACTER OF SOME OF THE PRINCIPAL FUNCTIONS OF ANIMAL LIFE.

By Richard Shettle, M.D., M.R.C.S., L.S.A.

In the year 1863 I published two papers in "The Lancet," stating, I had shown by the galvanometer that a current of electricity existed in arterial blood, which was not present in that drawn from the veins; that I attributed the bright colour of arterial blood to the presence of this principle, acting as a stimulant to the vital fluid and corrugating or drawing in the sides of the corpuscles; further that the phenomena of animal life depended upon the existence of electricity in the blood as a vital force; and lastly that coagulation was the result of this vital electricity compressing the particles of matter in the liquor sanguinis as it assumed a current form when passing away from the corpuscles.

Since that time I have frequently made similar experiments, and with the like result, except on two occasions, when the venous blood produced as strong a deflection of the needle as did the arterial; but in these instances the venous blood was of the same bright colour as the arterial, a fact upon which too much stress cannot be laid; and I now believe the anomalous effects to have been caused by a paralysed state of the peripheral nerves, preventing the abstraction of the vital force in the capillaries. A similar state of the blood occurs during sleep, in the hybernants during the period of repose, and in those dying of starvation. The two first would be instances in which there would be non-abstraction of vital force in the capillaries, from the normal suspension of the functions of the cerebral and sensorial ganglia, a state which is known to exist during sleep. But in those dying of starvation the peripheral nerves would be paralysed, partly from exhaustion of the vital force of the great nervous centres, and partly from the absence of carbon in the tissues; and this notwithstanding the generation of a vital force in the

lungs; for the nervous system, like any other part of the body, cannot carry on the functions of life unless it be supplied with nutriment as well as vital force. These experiments, however, simply referred to the action which takes place in the lungs during respiration, and in advancing these views with regard to electricity as a *vital force* existing in the blood, I desire that it may be clearly understood, that I do not seek to do away with the necessity for chemical action, but to call attention to the fact that under such chemical action, there must be a manifestation of electrical force, and that such force then becomes a vital force.

In the following paper I desire to point out the nature of digestion, and its similarity to ordinary galvanic action; the mode in which chyle is formed, organised, and converted into red blood; the electrical connection which exists between the stomach, spleen, liver, pancreas, and duodenum; and further, to show that the spleen is an electrical organ whose function is to destroy old blood corpuscles, and add to the organisation of new corpuscles; and also very briefly to touch upon the function of the liver.

It appears to me that the instrument known as Wheatstone's bridge, when in action and the four conducting wires are continuous, represents pretty accurately the state of the stomach and the organs with which it is connected by means of the sympathetic, when in their state of *inaction or rest.* An electrical current is known to be passing through the wires of the instrument and the galvanometer, but as perfect equilibrium exists, the needle is unaffected; but when a resistance is interposed in either of the wires, a deflection of the needle takes place by disturbance of equilibrium. So when ordinary capillary action is going on, the vital electricity is being abstracted by the nerve from the blood in the capillary, but without appreciable manifestation of force; but when capillary action is impeded or increased, there is a disturbance of electrical equilibrium by the interposition of a resisting force, and a corresponding manifestation of electrical force.

When food is taken into the stomach its first act is to cause an irritation of the peripheral nerves of the organ; in other words, electric equilibrium is disturbed, the nerves make a demand upon the capillaries, and a sufficient amount of blood is drawn to the parts to restore equilibrium. But in the case of digestion the impression is constantly being made, so long as food remains in the stomach to be digested; and as the food comes into the stomach, after it has undergone the process of mastication, mixed with soda, ptyaline (an alkali), common salt, and other condiments, or stimulants to the nerves of the stomach, it forms an exceedingly good electrolytic body, capable of generating an acid on the surface of the stomach, and so assisting in its own digestion or conversion into chyme.

The first act of the stomach is seen, then, to be simply increased capillary action and the formation of acid gastric juice; the second

and great act is the stimulation of the other organs with which the stomach is connected by means of the sympathetic, and the production of peristaltic action. It cannot be questioned but that these effects are the result of a still further disturbance of the electric equilibrium of the nerves. Thus, if the peripheræ of the sympathetic nerves of the stomach collect and convey force to the neighbouring organs, the peripheræ of the same nerves in those organs will stimulate the capillaries in each organ to increased action, and secretion will be effected at the expense of the *vitality* of the blood in the capillaries, and the electrical circuits will be established between these organs and the stomach and each other, partly by means of their respective secretions and partly by the nerves. Thus the stomach will act as the zincode or generator of force to the pancreas, liver, duodenal glands, etc., and its secretion is found to be acid, *i.e.*, the acid is making its appearance at the plus plate but negative pole, whilst these organs will be converted into the platinode, with the alkali appearing at the minus plate or positive pole. In the one case there would be repulsion of the particles of chyme from the walls of the stomach to the pylorus, and in the other repulsion of particles of pancreatic fluid, along the pancreatic duct to the duodenum, because bodies similarly electrified repel one another; but when these two fluids are brought into the duodenum, there would be an electrical affinity for each other, because they are in opposite electric states, and the acid chyme is thus converted into the alkaline molecular base of chyle. Of course the muscular contractions of the stomach, giving rise to peristaltic action of the organ, are due to a disturbance of the electric equilibrium of the muscular branches of the nerves, and must be the result of periodical discharges of force, the origin of which can be accounted for I believe in the peculiar organisation and action of the spleen.

ABSORPTION OF CHYLE.

When the minute particles of the molecular base of chyle are brought into contact with the villi of the intestine, the villi become erect and elongate themselves into the fluid, absorption of the fluid at once taking place. These villi are seen to consist of a central lacteal tube with open extremities, the walls of such tube being covered with a plexus of capillaries running from base to apex, so that the act of filling the capillaries with blood must tend not only to produce mechanical erection of the villi, but must at the same time serve very materially to dilate the tube, and such dilatation ought also to induce a sucking in of any fluid applied to the extremity of the villus. But I believe the great cause operating to promote absorption of the chyle by the villi is the difference in the electric state of the chyle in the intestine, and the blood in the capillaries of the villi; and although the alkalinity of the two fluids may differ but little, if at all, the blood is a highly organised fluid and

contains in its arterial state a considerable amount of electrical force, whilst the molecular base of chyle is only endowed with that amount of force which is inherent in every particle of matter, and consequently must present matter in an electro-negative state as compared with the blood in the capillary—*i.e.*, the blood in the capillary is acting as the zincode, and the chyle as the platinode. So that in the open mouths of these villi we have a ready channel for the negative chyle current to take, in opposition to the plus current of the blood in the capillaries surrounding the tube, the negative current being directed from apex to base of villus, thus causing absorption of the chyle.

Up to the present time physiologists have not been able to trace any nerves into these villi, but a layer of muscular fibre cells surrounding the tubes has been shown to exist by Kölliker and others, and these have been seen in the act of contraction during the absorption of chyle. Although nerve fibres may, and probably do exist, the mode in which we have seen the chyle to be absorbed by the villi appears to do away with the absolute necessity for the existence of nerves in the villi, even to produce contractions of the muscular fibres; for when a certain amount of chyle in an electro-negative state has been absorbed by the villus it will represent a force in the tube, which ought to give rise to a discharge of force from the capillaries, simply by the affinity of the two forces overcoming the resistance, under which muscular contractions will be set up. It appears, however, very probable that the villi are under the control of the ganglionic system of nerves, and if so, they would be influenced by the intermittent currents already alluded to as taking their origin in the spleen. From whatever source, however, the contractions of the muscular fibre arise, under the influence of such contractions there must be a manifestation of electrical force, and this force must always be developed in equal degree, being regulated by the amount of molecular base a villus can absorb at a time, and the plus current would be directed from the capillary to the molecular base; consequently when such manifestations of force take place there must be a compression of and union of the particles of molecular base, and a conversion of them into larger granules capable of acting as nuclei for the chyle corpuscles.

FORMATION OF CELL WALLS.

The molecular base of chyle is seen to consist of spherical particles of matter, which are supposed to have a coating of albumen. The tendency of these particles is rather to repel one another, than to unite, causing what is termed molecular action. Now molecular action appears to depend upon force emanating from the centres of the molecules and passing equally in all directions, and each molecule being similarly electrified would give rise to an equal amount

of force of an electrical character, and at certain distances from the centre of molecules mutual repulsion of force would take place, causing the molecule to rotate in all directions upon its centre, and giving rise secondarily to the formation of currents in opposite directions. It is to the influence of these currents that, I believe, the coatings of the molecular base are due. If this view be correct, it is evident that the amount of force emanating from a particle of matter must vary with the size and *character* of the molecule, but the structural character being the same, the force developed must depend upon the size of the particles. Consequently when the atoms of the molecular base have been combined in the process of absorption by the villi, so as to form particles large enough to act as nuclei for cells, the force emanating from them would be materially increased, and the *cell walls* generated by such force would be formed at greater distances from the nuclei, and correspondingly increased in strength also. It must be remembered that we are now dealing with matter in a state capable of permitting each particle to exert an influence over other particles, and thus displaying all the phenomena dependent upon its inherent force.

Mr. Lockhart Clarke has published some very valuable papers in the "Medical Critic" on the Nature of Volition, psychologically and physiologically considered, which appear to me to substantiate these views very materially. He says: "Every particle of matter, therefore, possesses an inherent principle or *capacity* of self motion, but whether this principle shall spontaneously result in *actual* motion or not will depend solely on the external and merely accidental conditions, of the absence or presence of resistance and restraint opposed to it by other forces. Now there is reason for believing that these external conditions may be so regulated and provided as to allow physical forces, or the principal of motion inherent in matter, not only to carry on these continuous operations which constitute life in an organism, but even to *originate* at intervals the *occasional* phenomena of outward action and locomotion." Vide "Medical Critic," April 1863.

Dr. Lionel Beale, in a paper "On Life" read by him at the meeting of the British Association held at Birmingham 1865, says "All growth, all active change, is due to the living or germinal matter, which is perfectly transparent and structureless; changes are not excited in this by external agencies, but the first impulse proceeds from within the living matter itself, and is in effect the operation of vital force or power." It would almost appear from the above that Dr. Lionel Beale is really but unconsciously describing the electric force itself, the analogy is so strong.

In illustration of the fact that discharges of electricity in the system will cause the compression of particles of matter, I cannot do better than refer to the formation of the solid which takes place under ordinary muscular contraction. Thus, when muscular fibre contracts there is a manifestation of electrical force, both in the

muscles and nerves, and matter existing in the fibres of the muscles becomes converted into a solid, losing at the same time all power of contracting again, and is then carried off as waste matter, although it may be destined to serve some other useful purpose in the animal economy. Although physiologists agree that there is an expenditure of vital force under the development of contractile power, they do not state, notwithstanding the discoveries of Du Bois Reymond, Matteucci, and others, in what that vital power consists. I therefore offer the following opinion as a solution of the question, so far as muscular action is concerned, which embraces in a degree, all other vital actions. During life the muscles and nerves are kept in a state of tone by a mutual action and reaction upon each other under capillary action. In a state of rest the nerve is acting as the platinode, and the blood in the capillary as the zincode, the plus current consequently being directed from the capillary to the terminal of the nerve, so causing nutriment to be deposited at the platinode, as in any other part of the body. But during natural action or contraction of a muscle, the nerve, by a discharge of force from the brain under the influence of the *will*, becomes converted into the plus force, and matter already existing in the muscular fibre is compressed and shut up in the form of a solid.

The following experiments also show the power electricity possesses of combining and arranging the particles of matter, and converting them into solid structures.

Experiment No. 1.—If water be mixed with the sap of plants, and the solution be carefully filtered through blotting paper, and a simple galvanic circuit be immersed, a very short time will suffice to form a film on the surface of the water. This film will gradually increase in thickness, and if examined microscopically will be seen to consist of minute particles of matter exhibiting vigorous molecular action, and although I have not absolutely seen cells formed in such specimens, I have seen cell after cell appear, and then, drawn by some invisible agency to a part where there had been previous accumulation of tissue, adapt themselves one to another so as to form tubes, apparently by a bursting of that portion of the cell wall which had adapted itself to the previous cell.

Experiment No. 2.—If we dissolve forty grains of gutta percha in six drachms of chloroform, put the solution in a wide-mouthed bottle, and then immerse a galvanic circuit and accurately close the mouth of the bottle with a cork, the gutta percha, which upon its solution was much lighter than the chloroform, very shortly acquires a higher specific gravity than the chloroform, and sinks to the bottom of the vessel. A good shaking will again cause intimate mixture to ensue, and before the gutta percha again sinks it will assume a flocculent appearance, and the movement of these flocculi will constitute a very delicate test of heat.

I now resume the consideration of the further organisation of the chyle.

The property of coagulation of the chyle appears to be coeval with the formation of chyle corpuscles, for before the chyle passes through the mesenteric glands it possesses feeble properties of coagulating, and the number of chyle corpuscles at such period is very limited. Now after the formation of cell walls, the force existing in the nuclei would still be exerting its influence upon surrounding matter, and when taken out of the body and allowed to remain at rest or when it is exposed to the influence of other matter upon it, (vide Mr. Lister's Croonian Lecture, delivered 1863,) currents would be given out in definite directions, and these currents would cause the compression of particles of matter, *because one form of electricity cannot be developed without the other, and the union of the two electricities would cause the formation of a solid*, in the same way as we have already seen it to be done in the ordinary action of a muscle. We can therefore readily understand that the amount of coagulability must correspond with the amount of organisation or vital force existing in the corpuscles. And, as I hope clearly to show when considering the function of the spleen, each time a corpuscle undergoes capillary action, *especially* if such takes place in a *glandular structure*, an addition is made to its contents and consequently to its inherent force, irrespective of any addition or abstraction from its vital force.

COLOUR.

But after the chyle has passed through these mesenteric glands it possesses a pale reddish *yellow* colour, (which we shall see must be regarded as an evidence of the amount of action going on in the gland,) and when allowed to stand for a time undergoes a regular coagulation, acquiring before it passes out of the thoracic duct a decided red tinge, which increases on exposure to air. I believe the production of this colour is *due simply to the action of electricity on matter*, and especially upon the salts of sodium, potassium, and iron; and the information we derive from the spectrum analysis very materially supports this view. Thus, the first change from white would be to a pale reddish yellow; the spectrum analysis tells us that in the production of colour the reaction of sodium is the most delicate of all metals, and Swan could detect by its means the 2,500,000th of a grain. Potassium and iron are less delicate tests, and give a red line, which colour we have already seen is not acquired by the chyle until shortly before it passes out of the thoracic duct, and has gained considerable property of coagulating.

SPLEEN.

After food has been taken the spleen commences to enlarge, and continues to increase in size for about five hours, when it attains its maximum size, that is about the time *when the process of chymification ceases*, the enlargement being caused by an accumulation of

blood in the substance of the organ; after the process of digestion has ceased it gradually decreases again, so that when digestion has terminated for some hours it contains very little blood, and is correspondingly small. It is believed that a certain amount of stagnation of blood occurs in the spleen during its state of action, and it is known that its blood, as compared with that of other organs, contains large quantities of iron and soda. It is known that animals may live after the spleen has been removed, but its removal has been followed in some instances by regeneration, and in others by enlargement of the glands of the neck and axillæ; Maggiorani has noticed a deficiency of iron in the blood corpuscles, and in dogs operated upon by Mr. Dalton, unnatural appetite and ferocity of disposition followed. As a natural sequence, its removal when not followed by regeneration must throw more work upon other glands that perform a somewhat similar office, but as other glands cannot act perfectly in such duty, we must expect corresponding changes in the blood, and also we must expect that the habits and disposition of the animal will be changed through the blood.

I proceed to show as briefly as possible the nature of the blood changes as they are known to occur, and the mode in which such appear to me to be brought about. (1) In the venous blood there is found to be a marked decrease in the total amount of solid matter, dependent upon the diminished proportion of red corpuscles. (2) A marked increase of albumen, to even double the previous amount. (3) Increase of fibrine almost constantly, and sometimes to even five or six times the previous amount. (4) The venous blood is remarkable for containing a larger portion of colourless corpuscles, believed to be derived from the white parenchyma, which it also contains; Hirt counted one colourless to 2,179 coloured corpuscles in the blood of the splenic artery, and one to 60 in that of the splenic vein. (5) The venous blood also contains a number of fibrinous flakes and peculiar cells (including rod-like crystals) of reddish yellow colouring matter, which seem to be red corpuscles in a state of degeneration. (6) Diffused amongst the colourless parenchyma (but in very variable amount) coloured cells are found, some of which are unchanged blood corpuscles, whilst others appear to be blood discs in various stages of retrograde metamorphosis, gradually diminishing in size, and assuming a golden yellow, brownish red, or even blackish colour, or having the pigmentary matter crystallized in rod-like form in their interior, or again breaking up into pigment granules.

MALPIGHIAN BODIES.

Before proceeding to show cause for these changes of the blood, I think it well to call attention to the splenic artery, which is remarkable for the rounded Malpighian bodies which are studded over its ramifications. It appears to me that these Malpighian

bodies perform a most important function in the action of the spleen, viz., that of accumulating and discharging electrical force, and thus acting as the prime conductor of the ordinary electrical machine; the spheroidal form causing the electric force disengaged in them to pass away in quantity and intensity, unlike ordinary capillary action which represents the discharge of electrical force quietly and continuously as it would be from points. Consequently the discharge from these bodies would represent on a small scale the discharge which takes place from the brain when we wish to move a muscle.

If we examine the minute structure of these Malpighian corpuscles we find that internally they correspond with the structure of the gland itself, but the walls are covered with capillaries, which also permeate their structure throughout. Their number and size correspond with the condition of the animal, being much the most numerous in animals that are well fed, and diminishing to a remarkable degree in those that are starved; thus showing that if they are not essential to the proper organisation of the blood, they are at least closely connected with it. Now if under ordinary capillary action, each capillary has its nerve, (as we have good reason to suppose is the case, for the point that is introduced into a capillary can scarcely draw blood without causing pain,) and the one is acting as the zincode to the other, the arrangement which is here seen to exist must entail electric force being given off with a certain amount of shock, because it is known that the Malpighian corpuscle is not represented by a corresponding enlargement of the nerve, and there must be some interruption to the current, for each wave of the blood in the artery would give rise to a wave of electrical force, and then a cessation, for the current of blood would not be as continuous here as it is in ordinary capillary action, and the current or shock of electrical force would be given off at the moment when the blood had filled the Malpighian corpuscle, that being the moment at which the greatest amount of force exists in the Malpighian body, and a portion of such force could not be given off without entailing a discharge of the whole amount.

FORMATION OF FIBRINOUS FLAKES.

I have already attempted to show that coagulation of the blood depends upon electrical force passing through the liquor sanguinis, but coagulation is also known to be favoured by rest, and if these Malpighian corpuscles act as I suggest, we readily see a cause for the formation of the fibrinous flakes which are found in the splenic venous blood, viz., the passage of currents of electricity through the liquor sanguinis, whilst the blood is somewhat at rest, in the lacunæ or channels of the splenic structure. Nor does it appear unreasonable to believe that the *elaboration of albumen and fibrine in the liquor sanguinis amounts to more than different stages of the same*

process, the result of different degrees of electrical action upon the necessary elements existing in the liquor sanguinis, of which the formation of fibrinous flakes is the limit, representing in fact a process analogous to ordinary growth of tissue.

The second effect of these discharges of electrical force would be to act upon the corpuscles of the blood, producing, as I hope clearly to show, a destructive action upon those which have become changed in character by passing a certain number of times through the circulation, and are thenceforward no longer calculated to carry life to tissues, but they assist to organize those which are of more recent origin.

To make this clear I must define as exactly as possible the changes which must be effected upon the corpuscles as they pass through the systemic capillaries. I have already given an opinion that in capillary action, the capillary is acting as the zincode and the nerve as the platinode, but this action might be reversed without detriment to these views in the main; for the diamagnetic* property it would acquire under the absorption of carbon would alter its character as an electric, and thus render the corpuscles incapable of exerting a stimulating influence upon surrounding bodies. In this way we get a closed circuit established between the systemic and pulmonic capillaries through the medium of the nervous system. The current of the blood not only establishing a current of fluid to supply nutriment or matter to the tissues, but also supplying the nervous system with electrical force, which then acts, I believe, like a Deluc's dry pile.

Now the effect of establishing such a closed electrical circuit would be to produce electrolytic action at the poles or terminals; and taking the body as a whole, and not considering here the circuits which must be established in the brain and other nerve centres, the brain would act as the platinode, the cerebro-spinal nerves as the conducting wires, and the blood would represent the zincode, the current of course passing from the blood to the nervous system, of which the brain and all nervous centres would be storehouses of force similar to a Deluc's pile. Such would be the action going on, according to the known laws of galvanic or voltaic electricity, and it follows as a natural sequence that wherever a break occurs in the circuit, electrolytic action must be set up. Now this break does occur between the extremity of the nerve and the tube of the capillary, and the plus current passing from the blood, would carry nutriment with it to be deposited round the nerve or negative plate, the force itself for the most part passing on through the wire or nerve to the brain, whilst the negative

* The author believes (and is taking steps to prove how far he is correct in his opinion) that the *arterial blood is magnetic*, and *only becomes diamagnetic as the vitality passes off when abstracted from the body and coagulation takes place;* or when under the influence of the nerves in the body, *it loses its property of giving life to the tissues.*

current would pass not only to the capillary but to the blood corpuscle, carrying with it particles of matter negatively electrified, which would represent waste tissue. In this way oxygen would be carried to the tissues and carbon would be drawn away from them, and the arterial blood in its conversion into venous might be rendered diamagnetic by the absorption of the carbon, and the abstraction of a certain amount of oxygen. But other principles besides oxygen and carbon would be amenable to the same law, and especially the salts of the liquor sanguinis. Consequently each time a corpuscle passed through a systemic capillary an addition must be made to the solid in its interior, and thus the chyle corpuscle with central nucleus of small size will at length become converted into a solid mass; capable, it is true, of exerting a greater amount of attractive force for electricity, but unfitted to maintain the life of the tissues, because the amount of solid in the interior would retain such force by its own inherent force. Thus, closely examining the changes which must take place in the blood corpuscle under ordinary capillary action, the cause of certain discrepancies of opinion as to the corpuscles having nuclei with even double cell walls as supposed by some, and their being solid with an indefinable cell wall as supposed by others, is easily understood, and is doubtless owing to investigators meeting with corpuscles in different ages or stages of growth; and this view is strongly supported by the observations of Professor Bennett, who discovered that the colourless corpuscles of the blood were of two distinct sizes, the small corresponding in size with the nuclei of the larger ones.

The establishing of these closed circuits is of immense importance with regard to the production of sensation, for we find that sensation is the result in all cases of an interruption to the current by a resisting medium, for the exemplification of which I must again refer to Professor Wheatstone's bridge. Thus, pressure upon the capillaries or nerves of the finger will interrupt the current, and give the sensation of touch. A ray of light thrown on the retina produces from the same cause an effect upon the brain, and the form of the object will be defined by the mode in which such rays fall upon the retina. Neither are we without proof of this fact: for if a ligature be applied to a nerve, and the nerve irritated above the ligature, that is towards the brain, no contractions ensue, but the animal exhibits signs of suffering; on the other hand, however, when the nerve is irritated below the ligature, strong contractions of the inferior muscles take place, without suffering being evinced. In electro-telegraphy we find that if a message be sent by a wire from a certain point to a distant station, say from London to Manchester, a continuous insulated conducting wire must extend between the instrument and battery in London and the instrument in Manchester which is to receive the signals, and there must also be a continuous conducting communication to

complete the circuit between Manchester and London. It was discovered by Steinheil that the earth itself may be made a substitute for the second conducting wire, thus proving that by creating a disturbance of electric equilibrium at a given point, the return current, notwithstanding its passing through so great a space as that of the earth, makes its appearance at the point at which primary disturbance had taken place. Now if we let the grey substance in the spinal cord represent the wire conducting an impression from a peripheral nerve to the brain, we shall at once perceive the use of the two roots of the spinal nerves, viz., to separate the afferent and efferent fibres. And supposing the brain and spinal cord to represent an electric circuit completed through the medium of the anterior and posterior roots of spinal nerves, the ganglia developed on the posterior root would act as an accumulator of force, in a similar way to a Leyden jar or Deluc's pile; and an impression being made upon the peripheral nerve by an interruption to the current, such impression would be conveyed first to the grey matter of the cord by the posterior root of the nerve, and thence through the grey matter to the brain, when a sensation would be produced answering to a deflection of the galvanometer needle, and a corresponding amount of electric force at once transmitted from the brain, through the motor tracts to the same point in the spinal cord where the impression had first been made. Such electric force would then, if necessary, be distributed to the muscles of the parts from which the impression had originated, and thus electric force would be converted into motor force.

For the purpose of more conveniently describing the causes of the changes which the blood undergoes in its passage through the spleen, it will be better to consider the corpuscles as existing in three different classes or states, but of course they would exist in every stage of transition, from the lowly organized chyle corpuscle, to the effete blood corpuscle.

1st.—Those recently formed, which have only passed once through the pulmonic capillary, and, not having their own organization complete, are consequently unable to carry vital force to other structures.

2nd.—Those which have become solid, and are no longer capable of acting as dispensers of vital force.

3rd.—Those which are in an intermediate state, and possess this power in the highest degree.

When a charge of electric force is given off by a Malpighian corpuscle in making its way to the nearest nerve fibre, and from thence to the nearest ganglia of the sympathetic, it would act upon the different classes of blood corpuscles enumerated above according to their state. Now there are two reasons why this plus charge from the Malpighian corpuscles, should select this second class of blood corpuscles in preference to either of the other classes, first, the amount of solid in them exceeds the amount in the others; secondly,

the solid contents of the corpuscles have been built up from their first passage through a systemic capillary by a negative current from the nerve, so that they would represent, not only a greater amount of matter, but that matter also in an opposite electric state. Now the particles of iron and soda, (both electro-positive bodies,) which we have already seen to exist in considerable quantity in the spleen, would possess an attracting influence for the nerve current; and as the third class of corpuscles as arterial blood possess a plus current, the abstraction of such current from them in their conversion into venous blood, would cause a negative current to flow into them. This would carry with it the iron and soda, thus adding to their contents and to their property of acquiring organization. But as the first class, or most recently formed corpuscles, exist in a negative state, they could scarcely derive any force from the nerve, although they might and probably do take up iron and soda by their own inherent force. Possibly also small corpuscles are here converted into larger ones by a similar process to that I have described as taking place in the absorption of chyle; still the fact of their being so converted would not give them colour until they had again passed through the pulmonic capillary, and had had such elements acted upon by the electric force. It must be evident that the effect of the plus current from the Malpighian corpuscle on the blood discs already converted into solids must be of a destructive character, causing the rupture of the cell walls and disintegration of the contents; and the electric force is quite capable of arranging some of the particles of matter thus set free, and converting them into crystals; whilst other atoms (such as uncombined iron) would be separated, and thus enabled to assist in the organization of the other two classes of blood corpuscles. This destruction of the coloured corpuscles would materially alter and *increase the proportion* of the colourless corpuscles. Now all these changes which the blood undergoes in the spleen would be greatly assisted by the comparative state of rest or slow movement the blood assumes in its passage through this important organ. But in addition to the changes effected in the blood of the spleen, and already commented upon, we ought to have the temperature of the splenic venous blood higher than that of the arterial, for we find that the passage of electricity through conductors is attended with the evolution of heat, the rise of temperature being proportional to the square of the quantity transmitted in equal times. Now we really do get this elevation of temperature, for the splenic venous blood is, with the exception of the blood of the hepatic vein, hotter than any other blood in the body, a very significant and instructive fact. Regarding the spleen as a whole, we find that in its active state it must be a powerful electro-positive body to the neighbouring organs, and upon due consideration we see that the Malpighian corpuscles represent in some respects the gland in miniature. Thus

they are the originators of interrupted currents of electrical force, minute, it is true, to a degree, but the discharge from all these corpuscles occurring at or about the same time, must in the aggregate give rise to the production of very considerable force, commencing, I believe, the *peristaltic action* of the stomach, and regulating the amount and moment of the discharge of the secretions of the pancreas, liver, duodenal glands, etc.; whilst by causing muscular contraction in the villi of the intestines, it would most materially assist in pumping chyle into the lacteals; but the formation of secretion by the liver, pancreas, etc., appears to me to result from a stimulus sent direct from the stomach to these various organs.

LIVER.

The mechanical changes that the blood undergoes in its passage through the liver may be easily understood if referred to *electric* action. Thus it possesses little if any power of coagulating; the coloured corpuscles are flaccid, of a violet hue, and show but little tendency to a rupture of their cell walls, the colourless corpuscles are seen to exist in very irregular forms, and, as I have already stated, the blood of the hepatic vein is of higher temperature than any other blood in the body. The blood of the hepatic vein, as compared with that of the portal vein, contains more fat and sugar, and is far poorer in water; the cells contain less fat, less salts, and especially less hematine or at least iron. This blood is, however, somewhat richer in extractive matter, and the specific gravity of the cells is higher than that of the cells of portal blood.

With regard to temperature, we have already found that the temperature of the blood was raised by its passage through the spleen, and have referred such to an electrical law. It consequently follows that the blood of the vena portæ, from the admixture of the splenic blood with that from the gastric and mesenteric source, must reach the liver at a higher temperature than venous blood usually attains. This is one cause of increased heat, but the great cause of the blood coming out of the liver hotter than it passes into it, is doubtless owing to the large amount of electro-chemical action which takes place in the gland, combined with the *very impure state of the blood* after it has been broken up in the spleen. And this opinion is confirmed by another electrical law, for we not only find that the passage of electricity through conductors is attended with the evolution of heat, but that the amount of heat developed *is inversely with the conducting power*. It necessarily obtains that if electrical action goes on in any form in the liver, the blood, with the large amount of *débris* it contains, must afford that amount of resistance to the passage of the electric current which would cause this development of heat. Again, the large amount of action that is known to take place in the liver, if of an electro-chemical nature, must be carried on in great measure at the expense

of the vital force of the blood, a large portion of which has already been deprived of its vital power; and as the amount of chemical action would correspond with the amount of electrolytic action, it necessarily follows that the blood must be most completely deprived of all vital force in its passage through the liver. For it must be remembered that in this organ we have a very different apparatus to that which exists in the spleen, the liver appearing to be constructed upon a principle especially adapted for favouring capillary action. Indeed, so close is the arrangement of hepatic tubes, that when the vascular network is injected, as it must be when secretion is going on, no vacuities or interspaces can be seen. By a complete removal of the vital force of the blood, we establish a cause for its non-coagulability, and for the flaccid state of the corpuscles. But their violet hue is doubtless owing to their having imbibed matter through the negative current during capillary action, which would also add to their specific gravity; whilst their exhibiting but little tendency to rupture of their cell walls, would be a necessary consequence of the comparatively small quantity of water this blood contains. The irregular shapes that the colourless corpuscles assume would result from the removal of any force they may have acquired which might have a tendency to become converted into a vital force, and perhaps also of some of their inherent force. The large amount of electrolytic action here taking place would also be sufficient cause for the loss of water the blood sustains, and its elements are doubtless necessary for the elaboration of those chemical compounds formed in the liver, the consideration of which I cannot enter upon now, although it does appear to me probable that under electrolytic action such may be produced artificially, by a series of carefully conducted experiments. In support of this view of the cause of the loss of water, I must refer to some instructive and interesting experiments made by Helmholtz on the chemical changes induced in the tissues by muscular action. Powerful contractions were induced by electricity in the amputated leg of a frog and were kept up as long as the irritability was retained. The flesh of the two limbs was then analysed, and it was found that in every instance the water extractive was diminished in the electrized muscle, to the extent of from 20 to 24 per cent.

I cannot conclude this paper without referring to the discoveries Mr. Lockhart Clarke has recently made with regard to the pathology of diseases of the nervous system, and especially of tetanus, as these discoveries show, I believe, most satisfactorily the nature and cause of such diseases, and they afford absolute proof that electrical currents are constantly circulating in the nervous system. I cannot do better than quote his own words with regard to the morbid appearances he detected in one case of tetanus, which will be found to be a type of all. He says,—

"The sheath of the cord was natural in appearance, and had a reddish hue, owing to the fulness of the vessels on the sur-

face of the cord. Over its whole extent the cord was covered with large injected vessels, which were nearly as thick as whip-cord, they were near together, and ran more or less parallel to the length of the cord. The white and the grey matter were both congested, and the puncta were very conspicuous everywhere. In sections of this cord I found the grey substance in particular very much congested, and not only were the vessels unnaturally dilated, but each was more or less surrounded by a granular and originally fluid exudation, in which the natural tissue of the part became broken down and ultimately dissolved. In a microscopical preparation a large triangular mass of this exudation is represented where it occupies the bottom of the anterior median fissure, and has destroyed a part of the anterior median commissure, by extending to the right. The same parts of the grey substance of other sections are also represented more highly magnified, and in one figure a quantity of granular exudation has enveloped and partially destroyed some blood-vessels and the pia mater which supports them. The exudation extending round the bottom of the anterior column, destroying a portion of the anterior commissure, and following the course of an evidently diseased blood-vessel into the middle of the anterior cornu, where it has destroyed a part of the grey substance," &c. &c.

I need not carry the quotation further, for if these morbid appearances prove anything at all, they prove that the nervous system has made greater demand upon the blood vessels for nerve force, than those blood-vessels, under ordinary capillary action, are able to supply; and consequently a rupture not only of the capillaries but of the larger vessels also takes place, by which means the substance of the cord itself becomes disintegrated; and the effused portion would render it an inferior conductor of impressions or nerve force, be that force what it may. But we really have no difficulty in determining the nature of this force, for by reference to an experiment of Du Bois Reymond's, which may be easily repeated, we find that the slightest scratch on a finger will cause a deflection of the galvanometer needle. Du Bois Reymond conducts the experiment as follows. "The forefinger of each hand is dipped into the conducting vessels, so that the two arms are included in an opposite direction in the circuit of the galvanometer, even before any contraction of the muscles is made, at the moment when the circuit is completed the needle is more or less deflected in one or the other direction. This deflection is caused by some heterogeneity of the skin of the fingers, and up to the time when Du Bois Reymond published his paper on the subject he had not determined the law of its production." (*I would however venture to suggest that it is due in great measure to some inequality of force in the two sides of the body.*) "If there be ever so slight an abrasion of the skin of one of the fingers the deflection is said by Du Bois Reymond to be incomparably greater, and the current is directed through the galvano-

meter from the finger which was hurt to the unhurt one. In other words, if instead of the human body a heterogeneous metallic arch were placed with its two ends in the two conducting vessels, the wounded finger would act like the zinc or positive metal, and the sound finger like the platinum or negative metal. When one of the fingers has an abrasion, the permanent deflection which remains is so considerable that it interferes with and prevents any more delicate observations." The above is quoted from Dr. Bence Jones' work on Animal Electricity.

This deflection of the galvanometer needle gives positive proof of the passage of an electrical current from a wound, and the greater the extent of such wounded surface the greater the amount of electrical force given off. Now by lacerations of the nerves or simple irritation of them, as in idiopathic and hysterical tetanus, the same effect is produced, but whilst in simple irritation of the nerves the tetanic spasm ceases soon after the source of the irritation has been removed, provided such irritation has not been too long continued, in laceration of a nerve we generally get fatal results after the disease has been once thoroughly established.

Now if electricity be the real nerve force, and if there be continuous currents passing to and from the nerve centres and the peripheral extremities of the nerves in their state of rest, keeping them in an electrotonic state, then any injury of a nerve, by means of which continuity of current would be interfered with, would cause the nervous centres to take on action, and we should get electrical force manifested by contractions of the muscles. In fact, the injury of a nerve would act precisely in the same way as Wheatstone's bridge with a resisting medium always interposed in one of the wires. And the result of this excessive action upon the nerve centres would be primarily to exhaust nerve power ; and secondarily to cause all those lesions of the nerve centres which Mr. Lockhart Clarke has shown to exist in this disease, by nature endeavouring through the blood-vessels to supply the force in sufficient quantity to meet the increased demand, but failing in such effort the vessels give way with the above results.

In thus imperfectly sketching the mode in which it appears to me the food is disintegrated and converted into chyle, and subsequently into *organised red blood endowed with a vital force*, I have endeavoured to follow as nearly as possible that course which should prevail, if that *vital force* be really *an electric force.* For it is clear that if such be the case the various processes and functions of life must be carried on and governed by *the same laws that govern electricity in general.* At any rate, the laws which have already been found to obtain cannot be set aside, although our knowledge of those laws may require to be somewhat *enlarged.* I especially refer to the conversion of electricity into a vital force. It is far from my wish, however, to assert dogmatically that the system I have followed is strictly correct, for the capillary may be acting as the platinod e

instead of the zincode to the nerve, and we know that the terms plus and minus as applied to the electrical forces are only names used to distinguish the two currents, one of which cannot be developed without the other manifesting itself in equal degree. In other ways also we may have to reverse electrical action, but such really is of no importance with regard to these processes being governed by electrical laws, and are only details of the plan requiring to be worked out. My great object in writing this paper is to draw attention to my experiments proving the electrical character of the arterial blood, that such experiments may be repeated and investigated by competent and unprejudiced persons, when, if found to be correct, all these details will follow as a matter of course. It is well known that in the earth itself electric currents are constantly circulating, and that the amount of these currents varies in different localities. It is also well known that bodies hold different positions in the scale, by which means they are rendered positive and negative to each other, and although not insulated from each other, their normal position in such scale is not changed, unless it be under electrolytic action (I do not, of course, here refer to free electricity such as we get displayed in the various phenomena of atmospheric electricity) or to the decomposition of one of the elements, for the carrying on of which process a definite arrangement of the bodies is essential. It is also known that some bodies are *magnetic* and others *diamagnetic* only, and the structural arrangement of their component particles appears from the researches of Tyndal and Knoblauch to be one great cause of the phenomenon; still such property is retained in spite of their being uninsulated from each other. Now as the animal frame is built up of matter which we have already seen to be subservient to the laws above named, we may fairly assume that the same principle may be acting upon matter in the body as upon matter out of it. And even with much greater effect, because the animal structure is so built up of solids and liquids that it would appear to be purposely constructed to favour electrolytic action. If the blood be charged with a plus or minus force, and such force be given out in capillary action, then electrolytic action must go on, and this electrolytic action may convert the venous blood into a diamagnetic body.

By referring to animal chemistry we find that the brain contains a large amount of phosphorus, the proportion being from 1-20th to 1-30th of the whole solid matter, a body standing high in the scale of electro-negative elements; and as all nerve fibre partakes of the same chemical characteristics, we can at once assign a reason for the electric force of the blood being abstracted by the nerve under capillary action. We are thus taught by animal chemistry the change that must be effected in the electric state of the corpuscles and the various tissues and secretions, and there can be no question that a careful study of animal chemistry will practically do very much to simplify the treatment of disease. We can thus ascertain

that which is deficient in the system, or that which by its presence may give rise to abnormal action by retarding electric or magnetic action, one or both. In the one case we shall add the principle which will promote normal conductivity, in the other we shall employ remedies to abstract that which is acting on the animal machinery as impurity does upon conductors generally, and thus restore normal action. It cannot, however, be denied that in thus considering the vital processes which take place under the phenomena of *animal life*, we reduce those processes to the action of certain physical laws; but I trust no one will on that account look with irreverence on the *one Great Cause acting as the mover* of all, and upon this point I believe it is due to myself, and to others who may enter upon the consideration of this grand subject, to be very explicit; it is expressly written in the Divine Word, that "the blood of it is for the life thereof," and irrespective of the lesson we are taught by the extreme beauty and order of the mechanism employed in the processes of life we have been considering, and which must establish the wisdom and power of the Creator, I believe I have here proved these words to be strictly correct; and in attributing the phenomena of animal life to *continuous currents of electricity never ceasing without a corresponding cessation of the functions of life*, I have as precisely as possible defined the nature of animal life, and thereby have opened out a vast and most interesting field of research, the important bearing of which upon the scientific study and practice of medicine, cannot in my opinion be over-estimated.

ON THE DETECTION OF THE ALKALOIDS BY THE MICROSCOPE.

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The processes already in use for the detection of minute quantities of the alkaloids in cases of suspected poisoning, are not so delicate and so certain as to render useless the search after other methods. The subject is one which from its vital importance to the public and its own intrinsic interest well deserves the careful study of the medical man; and at the present time it is obtaining much attention. M. Helvig in Germany and Dr. Guy in England have by their investigations attracted the notice of toxicologists to a new and ingenious process for the recognition, by means of sublimation, of very minute quantities of the alkaloids. If I venture to offer for your consideration some experiments of my own on this matter, and some criticisms on the process and its value, I would do so in no spirit of dogmatism or presumption, but in that of a humble, earnest seeker after Truth, aiming to see and be guided by her alone; and if I am able to set before you facts which justify a distrust of the sublimation process as a toxicological test, I hope not to stop there, but to indicate, even if it be only faintly, a form of micro-chemistry which may prove to be uniform and trustworthy in its results. This is a wide field, little cultivated, and I profess to have done no more than to have delved in a narrow corner of it. Whether the same place has been cropped before, I know not; but I am not aware of any harvest of this kind which has been gathered from it, and sent to market for general use.

I will first briefly summarise the observations and opinions of M. Helvig and Dr. Guy, more especially as set forth in the very elaborate papers of the latter gentleman in the "Pharmaceutical Journal" for June 1867, and the three following months.

It had long been known that many solid substances, organic and inorganic, would pass into vapour on the application of heat, and would re-form as solids on a neighbouring cooler surface; in other words, that they would sublime. Among these substances are camphor, benzoic acid, theine, sal ammoniac, iodine, arsenious acid, and many others. It was the knowledge of the value of sublimation as applied to the detection of the last-named substance that led

M. Helvig to attempt the same process in the case of the alkaloids. I need not describe the manner in which he and Dr. Guy obtained their sublimates, but will relate the method I have myself adopted, which differs in some minutiæ only from theirs, and is I think more exact. It is a process which has been elaborated by my friend Mr. Waddington, whose attention to microscopic chemistry for a long time past enables him to speak with authority on such matters, and by whose kind permission I am enabled to make use of a paper on the best method of obtaining sublimates, which he purposes shortly to read before the Pharmaceutical Society. He says the process which he has adopted is of a most simple character, the only apparatus necessary being an ordinary gas furnace or a spirit lamp, a piece of thin sheet iron, a few microscopic glass slides, and some glass rings such as are used for making cells. The iron plate is supported over the heating apparatus, and a glass slide is laid upon it; in the centre of this is placed a ring, within which is put the substance to be sublimed, and over all, another slide. The heat is then applied, and sublimation commences in a longer or shorter period. Three lines should be drawn with a scratching instrument across the iron plate, one in the middle, the others an inch and a half on each side; the centre of the slide will thus be immediately obtained. The plate should be slightly concave from side to side, so that a three-inch glass slide when placed upon it will not touch in the centre by one-sixth to one-eighth of an inch. Heat is thus much more equably applied, and as that part of the glass which carries the substance experimented upon does not come into immediate contact with the iron plate, there is less risk of damage from too high a temperature. The glass on which the sublimate is received should not be a thin glass cover, but the ordinary slide, for these reasons—1st, as a rule much better sublimates are obtained when the vapour is condensed on a hot than on a cold glass, and as the glass covers are very thin they cool much more rapidly than a slide; 2nd, they are more easily handled; 3rd, chemical reagents are more easily applied to them; and 4th, the sublimate when mounted as a permanent object is seen from the best side, for it is viewed from the free and not from the attached surface. By this means arsenious acid may be sublimed with ease and certainty in its characteristic octahedral crystals. One thing is necessary, the covering slide must be hot before it is put on; if it is too cool the sublimate will form in granules; and it should be left on some time. As a rule, the longer it remains the larger and finer are the crystals. When an organic substance is to be sublimed, it is well to mix it intimately with five or six times its bulk of powdered glass; by this means it is more minutely subdivided, and in some measure is protected from destruction by too great a heat. Powdered glass is better than sand for this purpose, as it is more easily cleaned, and can be obtained in a finer state of division. As a rule, the nearer the receiving

glass is to the subliming substance the better; one-eighth or one-twelfth of an inch is a good distance. The heat must be carefully regulated or the alkaloid may be destroyed, and this may be easily done, if the iron plate is large enough, by moving the lamp towards one end of it. The receiving glass should only be a few degrees lower in temperature than the glass from which the substance is sublimed, and no artificial means of cooling it should be adopted.

The practical results of this process are on the table; and it will be seen that they fully bear out the assertions of M. Helvig and Dr. Guy that by it very distinct and beautiful crystalline forms may be obtained.

Thus far the matter is clear; I would now consider some further questions in connection with it.

Both M. Helvig and Dr. Guy conclusively prove that very small quantities of the alkaloids can be sublimed, and the latter believes that "a quantity so small as the 1,500th of a grain would yield abundant evidence of the presence of strychnia." As a physical and chemical fact, the delicacy of the process is of importance. As a toxicological fact its value is less certain. It is not yet determined what amount of reliance would be placed by the law on experiments dealing with such minute fractions of a grain. I can conceive a counsel for the defence making this a telling point with a jury, in the absence of other conclusive evidence. Dr. Guy then shows that a sublimate can be obtained from a solution of the alkaloid in benzole. He afterwards considers the question, "Is sublimation a property of the alkaloids and the allied active principles as a class?" and answers that as a general rule it is, and that the salts of the alkaloids will also sublime. He then observes that the joint use of the microscope and of chemical appliances is necessary, and describes the apparatus required. He relates the behaviour of the substance undergoing sublimation: "It may be driven off without undergoing change or leaving residue, and the disc may be covered with crystals or an amorphous sublimate; it may coalesce, throw out long silky crystals to be gradually transferred as crystals to the glass disc, and it may melt with or without previous change of colour, retain or shift its place, deposit carbon more or less abundantly, and yield a sublimate of detached crystals, twigs, tufts, branching patterns, watered patterns with or without crystalloids, the melting and deposit of carbon being common properties of the alkaloids and of some analogous active principles." He mentions the different alkaloids which give these different results. On this paragraph I would here merely remark that deposit of carbon simply means the destruction of the substance under examination, and that the specimens on the table show that one alkaloid will give many different forms of sublimates. "The microscopic examination of the sublimates," he says, "comes next in order. Then follows, as the last step in this mixed method, the

action of reagents," and he describes under the three heads the precautions necessary to be observed.

I pass by the remarks on the sublimation of blood, colouring matter of urine, etc., as these have nothing to do with my present purpose, the microscopic detection of the alkaloids; and because I believe there is nothing to be said about them at present except that both watery and oily matters may be driven off from them, and condensed in fluid globules. If the heat is carried far enough, carbon of course will also be found.

With a summary of Dr. Guy's remarks on the reagents to be used I conclude this *resumé*. In the first place, he says that it is necessary to be careful not to mistake the crystalline form of the reagent for crystals resulting from the union of the alkaloid with the reagent; as well as crystals of the alkaloidal sublimate, which may form from the solution independently of any chemical action from the substance applied. He advises the use of solutions of moderate strength, and mentions carbazotic acid, bichromate of potash, red prussiate of potash, and nitro-prusside of sodium, as the most useful reagents; and it is clearly to the appearances resulting from these reagents that he attaches the greatest diagnostic value. But this part of the process is not sublimation, has no necessary connection with sublimation, and does not need sublimation as an antecedent.

The papers of Dr. Guy are well worth a careful study, from the minuteness of the details and the many practical hints given; and if I venture to adduce objections to his process and to express a different opinion of its value, I do so with the greatest deference and respect to him, and only because I think that my own observations and experiments lead me inevitably to such conclusions.

That the alkaloids will sublime, then, is certain; that they will always sublime in like manner, if the physical conditions are the same, is equally certain; and that the typical forms when in perfection may be recognised with considerable approach to certainty, is no less true. But the value of the sublimation process as a toxicological test is not to be determined by such considerations as these. We must pass in review the weak links of the chain, the difficulties and doubts, in order to determine the amount of reliance that may be placed on the process. The objections are of various kinds. In the first place, the alkaloid may be destroyed; many of these substances offer great resistance to the destructive action of heat; but all, in common with every organic crystallizable material, can be decomposed by heat, and the sudden flaring of a lamp, or the accidental inattention of an operator, may make all the difference between an organic poison and a harmless mass of charcoal.

Again, the alkaloid may be lost; when in vapour a breath or a puff of air may blow it through the chinks and crevices of the apparatus, and, as far as my experiments go, in no case can the whole amount of the substance which is undergoing sublimation be collected

on the upper glass; some of it invariably escapes. Operating as is generally necessary in toxicological investigations with very small quantities, this loss may be of great moment.

But a much more important series of objections has now to be considered; objections which take their origin in the physical conditions under which the sublimation is conducted, and which are not in all cases under the control of the operator.

We have admitted that some of the perfect forms of crystals obtained by sublimation may be diagnostic of the alkaloid from which they are derived, but what about the imperfect ones, and it is with these that we have most commonly to deal; in Dr. Guy's narrative it will be found that the least common result is the production of perfect crystals. Every alkaloid may be sublimed in many forms, a perfect typical crystal on the one hand, and an amorphous granular mass on the other, with any number of intervening shapes, and very many of these substances have forms common to two or more of them. I have slides in my possession showing the very great similarity and almost identity of shape in the cases of morphine and picrotoxine, strychnine and santonine, salicine and picrotoxine, strychnine and hippuric acid, and some others,—and in one remarkable instance it is absolutely impossible to recognise any difference between a slide of square blocks of codeine and one of similar forms of santonine, and both verge very closely on an almost identical slide of strychnine. But most important of all is the likeness of certain crystals produced by the sublimation of strychnine to the octahedral crystals of arsenious acid; they are hardly to be distinguished. The similarity of certain forms of morphia and picrotoxine sublimates seems to me most important. What more common than to find beer adulterated with cocculus indicus; what more common than to find a person, dying under circumstances leading to a suspicion of organic poisoning, who has recently drunk malt liquor. How easy it would be for a toxicologist, relying on the accuracy of the sublimation process, to persuade himself that he had found a cognisable amount of morphine when it was really picrotoxine, demonstrating not a criminal poisoner, but a fraudulent beer-seller. The appearance of the sublimate itself, then, depends more on the physical conditions under which it is produced than on the chemical composition of the alkaloid from which it is obtained. It does not under all circumstances denote, with the unerring certainty required in matters of this importance, the identity of the substance from which it is derived, and if it is not invariable it can only be delusive.

But how does it behave with reagents? Granting that the form of the sublimate is no certain test of the originating alkaloid, does this further step give the desired knowledge. To pronounce judgment on this matter we must place clearly before our minds the way in which chemical tests act. And this appears to be solely in one of two ways: they may produce or destroy or change colour, and

they may throw down or dissolve a deposit. For micro-chemical purposes, colour is of comparatively little value, and certainly cannot be relied on; for just in so far as the substance is magnified, in the same ratio is the intensity of the colour decreased; the magnifying of the object and the application of a colour test tend to mutual destruction. What then is to be said for the other mode of action? The mere solution of the sublimate is of no moment, it is the deposit from the solution which is material. Now a deposit may be of two kinds, amorphous and crystalline. The former must be put out of consideration, for an amorphous deposit is only an amorphous deposit, from whatever substance it may have been derived, and can have no signification. We are reduced then, to the crystalline form, and here I believe we arrive on firm ground. Micro-crystallography is a subject little worked yet, but which will repay a careful observer for any trouble he may expend on its investigation. But here a caution is necessary: different substances will, under certain circumstances, crystallize in such a manner that they can with difficulty be recognised from one another. Some salts of analogous bases are so nearly alike as to be quite undistinguishable; but this similarity of shape does not extend to all salts, an example of which I hope in the sequel to demonstrate. But for the production of crystals of the alkaloids from chemical substances applied under the microscope, no sublimation is necessary; nay, if the considerations I have brought before you be, as I contend to have shown that they are, valid and true, sublimation is a let and a hindrance to the analyst instead of a guide and an aid. To produce crystals of a salt of the alkaloid by the application of a reagent, the base must be dissolved in it or in some other fluid; there must be solution of the old before crystals of a new compound can form. But the alkaloid, if there be one, is already in solution in the suspected substance, or may be dissolved out of it directly. Why then dry and sublime, with the necessity of re-dissolving the sublimate in order that the test may be applied, when what you want is already done, without risk of destruction by the incautious application of heat, or chance of dissipation when in vapour? The alkaloid is there in the mother liquid, or the dry condition, to be dissolved in whatever medium may be thought best, and then and there ready for the micro-chemical test; why interpolate another process with its attendant risks and dangers in order to return to the spot from which we set out? I can see no advantage, but every disadvantage. I would, then, discard sublimation as a poison-detecting process. There are doubtless many chemical agents which, when added to an alkaloid, produce definite and notable changes; and without depreciating the worth of the results obtained from the tests which Dr. Guy has proposed for application to the sublimate, I would suggest the iodo-sulphates, or as they should more properly be called, the sulphates of the iodo-alkaloids, to your serious considera-

tion. The alkaloids in general form very numerous compounds, and especially they unite with other bases to form compound salts. A salt of quinine of this nature, the sulphate of iodo-quinine, has been carefully studied by Dr. Herapath, and indeed has been called after him, Herapathite. He has formed similar salts with the other cinchona bases, and has proposed this plan as a means for detecting quinidine in quinine, and cinchonidine in cinchonine. The sulphate of iodo-quinine may be obtained by dissolving the acid sulphate of quinine in warm acetic acid, and adding to the heated liquid an alcoholic solution of iodine drop by drop. After standing for a few hours the salt is deposited in large flat rectangular plates, which when seen by reflected light are of a brilliant green colour, with a metallic lustre like the wing-covers of cantharides. When viewed by transmitted light, they appear to be of a pale olive tint, but the light so transmitted is perfectly polarized, so that if a second plate cross the first at right angles, the whole of the light is as completely arrested by the overlapping portion as it would be by two overlapping plates of tourmaline, the axes of which cross each other at right angles.

The alkaloids form with sulphuric acid two compounds, a mono-acid and a bi-acid salt. When the latter is dissolved in alcohol acidulated with dilute sulphuric acid and mixed with an alcoholic solution of iodine, a compound is formed in which iodine and quinine, together with water, form the base. This compound is easily crystallizable, has generally a marked colour, assumes a distinct and definite shape, and often polarises light. The sulphate of iodo-quinine may probably be taken as the type of the rest; it is formed from the simple salt by the substitution of two equivalents of iodine for two of water. The acid sulphate of quinine is represented by this formula—

$$C_{20}H_{24}N_2O_2\,H_2SO_4\,7H_2O,$$

and the sulphate of iodo-quinine as follows—

$$C_{20}H_{24}N_2O_2I_2\;\;H_2SO_4\;\;5H_2O.$$

Iodo-sulphates of many alkaloids have been formed as chemical curiosities, but I am not aware that they have ever been used for toxicological purposes. My attention was first directed to the subject by observing the very definite and characteristic rosettes of sulphate of iodo-quinine when formed on a slide under the microscope. I have now paid considerable attention to the subject, and have made very numerous experiments with the same result: a uniform production of definite and distinct crystals, differing largely in shape and size and colour, according to the alkaloid from which they are formed; in short, apparently characteristic of their chemical composition. As they are always formed under the same physical conditions, from

the conjunction of two solutions on the microscopic slide, there is no source of uncertainty there. The mode I have usually adopted is the following: the alkaloid is to be dissolved in alcohol or acetic acid slightly acidulated with dilute sulphuric acid; a portion of this solution is to be placed on a microscopic slide, and a minute drop of an alcoholic solution of iodine added. At first there will be mutual repulsion, but by and by, assisted by different positions of the slide, they will mix; if there is much fluid after it has spread over the surface of the glass, it is well to pour off the superabundance on to a second or even a third slide. In a few seconds, as evaporation goes on, the crystals will form. It would be useless to attempt a description of the appearances of the different crystals, but I have placed some slides of various iodo-sulphates before you, and must content myself with asking for them your critical attention. If further experiment and observation confirm my conclusions as to the practicability of the process, the uniformity of its results, and the characteristic differences of the several crystals, some modification of the recognised plans for treating substances supposed to contain organic poisons may be needed, so that the alkaloid may be obtained in the form of a sulphate; but this is a matter on which I will not enter now. The specimens which I have the pleasure to offer for your inspection are the sulphates of iodo-morphine, of iodo-codeine, of iodo-strychnine, and of iodo-atropine. These salts are all decomposable by heat, the iodine being driven off, and the simple sulphate left. If this is done by boiling, the alkaloid is neither decomposed nor dissipated. Up to the present I have had no time to investigate similar salts of the other less common alkaloids. The subject is new, and I offer these remarks, not as a complete account of the matter, but as a contribution towards that perfect knowledge which time will bring, and on which alone can the value of the process as a toxicological test be determined. I trust to be able at a future session to bring before you the results of more extended observations.

ON RIGOR MORTIS.

By Benjamin W. Richardson, M.A., M.D., F.R.S.,

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There is a common and predominating idea that the rigidity of animal bodies which follows death is brought on quickly by cold, and is retarded by warmth. This idea is based on the often observed fact that heat produces, as one of its most striking effects, fluidity of solid substances, flexibility, and softness. The object of this short essay is to show that in respect to muscular rigidity of animal bodies after death this notion is wrong, is indeed the reverse of what is true; to show that rigor mortis is kept back by cold, and is favoured by heat.

It has been known since, and it was surmised before, the time of John Hunter, that the process of coagulation of the blood is prevented by cold, and favoured by heat: and as rigor mortis and coagulation have been accepted, very widely, as analogous phenomena, it is singular that the relative influences of heat and cold on muscular rigidity have not been more faithfully recognised. John Hunter had undoubtedly an inkling of the truth, as one of his experiments testifies. He took three portions of fresh muscle of equal length, and placed them in waters of different temperatures: one piece was placed in water at 125°, another at 98°, and a third at 55°. The muscle in the water at 125° contracted immediately, so as to be half an inch shorter than the other two, and became "hard and stiff." The muscle in the water at 98° began to contract and grow stiff after six minutes, but at the end of twenty minutes was not so hard or short as the first piece. The muscle in the water at 55° began to shorten and grow hard after fifteen minutes, and after twenty minutes it was nearly as short and as hard as that in the water at 98°. At the end of twenty-four hours all the pieces were alike in length, and of equal rigidity. Hunter compared these facts with the facts he had observed in regard to coagulation of the blood, and the comparison was rigorous: but his observation was spoiled by the application, forced upon it, of his hypothesis about the abstract "life." He overlooked the simple physical influence of heat in following his assumed entity.

In the year 1859, while preparing the course of Lettsomian Lectures, delivered in 1860, I was struck by the following results of experiments.

(1) I observed, and I afterwards illustrated the fact in lecture, that if through a muscle, or set of muscles, in a living animal, a discharge be sent from a large induction coil, the muscles instantly are rendered intensely rigid, and remain rigid for many hours; and that if the animal be afterwards killed, the rigidity continues as if it were a rigor mortis preceding death, and only relaxes when the ordinary rigor, which is established after death in the other muscles, has naturally subsided.

(2) At the same time I observed the following fact. If, directly after the death of an animal, warm water not exceeding 110° Fahr. be injected into the arteries there is immediate and rapid contraction of the muscles, ending in quickly developed rigor mortis. But when the rigor mortis is in this way fully developed no further injection can be forced round the circulation: there is general infiltration of the connective tissue and cessation of motion.

(3) I observed that if, when the water is injected, its temperature be raised above 115° Fahr. there is the greatest difficulty in forcing it through the capillaries: in fact that there is contraction of vessels and resistance to over-heated fluids of all kinds: while cold fluids will run with the greatest ease and return by the veins almost instantaneously, causing no infiltration into the tissues.

(4) I learned that if an animal, after death, be allowed to remain exposed to a temperature above 65° Fahr. for the space of fifteen or twenty minutes, no active muscular contractions with alternate relaxations can be induced by injections with heated fluids; the heat causing a general contraction which becomes confirmed rigor mortis. But that if the animal immediately after death be exposed to a temperature at or below 32° Fahr. the injection of water at 115° will induce contractions and relaxations even as long as three hours after death, which contractions and relaxations will continue until all the muscles are fixed by the permanency of contraction, rigor mortis.

These observations naturally led me to make more precise researches on the induction of muscular rigidity after death. I proceeded in the following manner.

An animal, a rabbit, was killed with chloroform and the muscles of the neck on one side were subjected to the shock of a large coil giving a two-inch spark. These muscles passed at once into permanent rigor, the other muscles remaining flaccid. The flaccid muscles on the other side of the neck were then subjected to the shock with the same result. Next the muscles of each of the limbs were subjected to the same influence with the same result. And lastly, the muscles of the trunk on both sides of the body were treated similarly, and with similar recurrence of the phenomenon of contraction.

In a further experiment, individual muscles were selected, and the rigor mortis was brought on muscle by muscle.

The above experiments were repeated in another way. Heat was made to take the place of the electric spark. A rabbit just dead from chloroform was buried in sand heated to 110°, while a second rabbit destroyed at the same temperature was placed in a bladder and covered with ice. The animal at 110° was quite rigid in fifteen minutes. The animal in ice continued flaccid for one hour and twenty-five minutes, and then, the ice having melted and the temperature of the air being 58°, it became rigid slowly and irregularly.

A rabbit, also killed with chloroform, was placed on one of its sides on sand heated to 115°; in fifteen minutes it was rigid on the whole of the side that had been exposed to the heat, while the muscles on the side that had been exposed to the air were quite flaccid. After a quarter of an hour the muscles on the flaccid side were exposed to the heat, when they immediately became rigid.

Assisted by my friend, Dr. Sedgwick, to whom I am most deeply indebted for constant and able assistance, I let three rabbits sleep to death, simultaneously, in chloroform, and treated them as follows. One was left at a temperature of 65° with the skin of the body untouched. The second had the skin rapidly removed from it, and was left at the same temperature with the body surrounded by a thin layer of flannel. The third had the skin removed, and was left uncovered, at the same temperature. The result was thus faithfully chronicled:—

Rabbit number one was completely rigid in one hour and five minutes.

Rabbit number two was feebly rigid in one hour and a half.

Rabbit number three was quite flaccid two hours after death, and at no time became firmly rigid, the evaporation from the exposed surface acting as a means for causing reduction of temperature.

These experiments taken in their entirety prove, I think, to demonstration, that rigor mortis is accelerated by communication of the form of energy called heat, and is retarded or prevented by the rapid abstraction of heat.

This point seeming clear I was next anxious to ascertain whether rigor mortis, after it is established, can be removed by cold. To determine the point, a rabbit that had slept to death in chloroform was allowed to lie with the skin entire at a temperature of 70° Fahr., until every part of the body was intensely rigid. The chest was now laid open on the left side, and a tube was inserted into the aorta. Half a pint of distilled water was reduced to a temperature of 18° Fahr. by the addition to it of nitrate of ammonia, and at this low temperature the saline fluid was injected over the arterial circuit: the injection ran easily, and was followed by instant relaxation of all the muscles. In this experiment the nitrate of ammonia was carried in with the water, and thus complicated the ques-

tion;—Whether the effects were due *simply* to cold? Another experiment was therefore made, in which the cold was directly applied. The muscles of an animal rigid from death were laid bare in parts of the body, in the limbs, in the trunk, in the neck, and were subjected to ether spray until they were frozen: they were then cautiously thawed by being rubbed with ice, and at 38° Fahr. were entirely free from rigidity. The temperature was then raised to 100°, when they became once more rigid.

A frog treated with strychnia was thrown into a state of tetanic rigidity of the most determinate kind. It was next subjected to ether spray along the spine until all the body was frozen, when there was the most complete removal of the muscular rigidity. After an hour, on allowing the parts to thaw, there was first a want of contractile power under irritation, then a slight tremor, afterwards a more distinct contraction, and in the end a full return of the spasmodic paroxysms. In another experiment a frog was placed in water at 118° Fahr. There was immediate tetanic spasm. The animal was next subjected to the ether spray with direct relaxation.

Coincidently with the changes in the condition of muscle thus induced by the action of heat, there are other changes of a chemical kind of vast interest and importance. Thus, with the occurrence of rigor there is a change of chemical reaction of muscle; from being alkaline or neutral it becomes acid. The acidity is due to the presence of lactic acid which is formed in the muscle.

It remained to be learned whether the change from alkalinity to acidity was of necessity confined to rigor occurring after death. To settle this point the muscle of an animal under chloroform was laid open, and the muscular fluid was found alkaline to test paper: the muscle was then made rigid by shock from the induction coil, and with the fixed rigidity, although the parts were well excluded from the air, an acid reaction replaced the alkaline reaction, and lasted for eight hours, passing away only when the muscle once more became flaccid.

To produce acidity in a living muscle it is however necessary that the rigidity should be long sustained without relaxation. Alternate contractions and relaxations, however powerful the contractions, do not lead to acidity. To determine this point I took the reaction of the muscles of a toad: first, while they were healthy; and secondly, while they were contracting and relaxing under the tetanizing influence of strychnine. During both periods there was well marked alkalinity of the muscular fluid, and this continued until death. When all the muscles were permanently rigid, acidity followed in the usual way.*

*In this experiment, in which Dr. Sedgwick again rendered me his good services, we noticed a fact which deserves a special note. It was this. Whenever the skin of the toad was touched there was immediate and acute spasm, tetanic spasm, in all the muscles of the body; but when the exposed muscle was irritated in a similar way there was no such phenomenon. This fact is important as showing the entire independency of each muscle, and how distinctly local is the action of each under the influence of irritation.

The facts stated thus briefly, lead to one or two conclusions of great importance in respect to rigor mortis. I will place them in order.

(1) In respect to medico-legal practice. Rigor mortis will always be more quickly developed when the temperature of the day or night is high, or when the dead body is well covered.

(2) In respect to rigidity of muscle during life. When from any excess or accumulation of energy in a living muscle rigor of a persistent kind is induced in that muscle, there will follow the same changes as in the dead muscle, viz: development of an organic acid. Hence in muscles affected with rheumatic affection there is a direct cause for the development of such acid.

(3) The cause of rigor mortis is coagulation of the muscular fluid—muscle fibrine—and the excitant to the coagulation is heat. In brief, the force exhibited during life in the phenomena of voluntary and involuntary muscular motion, in the raising of water from the body, evaporation, and in secretion, is exhibited, on the arrest of these phenomena, in coagulation of blood and muscle. Thus rigor of muscle and coagulation of blood are truly the last evidences of the phenomena which in their totality we call life.

ON A NEW APPARATUS FOR THE ADMINISTRATION OF NARCOTIC VAPOURS,

AND SOME OBSERVATIONS ON THE VARIATIONS OF PULSE AND RESPIRATION DURING THE ANÆSTHESIA FROM BICHLORIDE OF METHYLENE (CHLOROMETHYL),

By F. E. Junker, M.D.,

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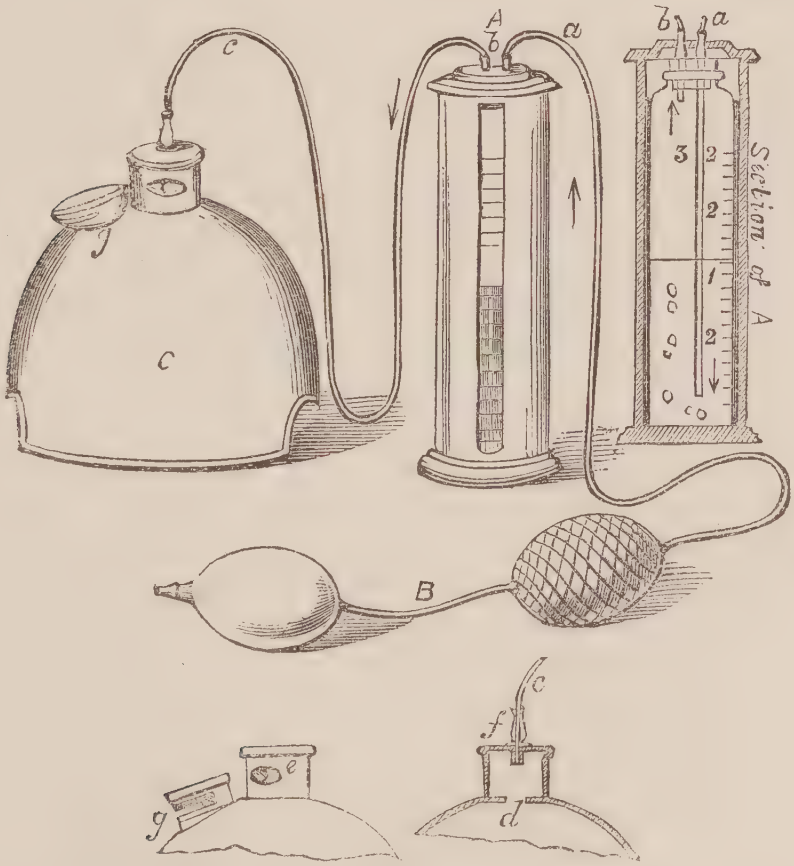
I beg to make known to the Profession an apparatus which I have lately devised, and have found very useful in administering chloromethyl and chloroform.

The apparatus is composed of three portions, the cylinder for the anæsthetic (*A*), the hand-bellows (*B*), and the mask or inhaler (*C*).

Fig. 6



The graduated glass cylinder (*A*) holds two ounces of fluid. It is closed by an air-tight top, through which two tubes enter, a

long one (*a*), which reaches nearly to the bottom, and a short one (*b*), which ends at the lower level of the stopper. The hand-bellows (*B*), which are the same as those used with Dr. Richardson's spray-apparatus, are connected with the long tube (*a*), and force the air into and through the anæsthetic fluid as indicated by the direction of the arrows. The air, impregnated with the vapour, passes through the short tube (*b*) into the inhaler (*C*), by means of an elastic connecting tube of convenient length. The glass cylinder fits into a stand made of a non-conducting material, which is necessary to prevent the chloromethyl from boiling when used in a hot room. The stand has a graduated side-slit, in order to enable the quantity of the anæsthetic used to be observed. The cylinder ought not to be charged with more than one fluid-ounce of the anæsthetic, in order to avoid a portion of the fluid escaping through the connecting tube whilst working the bellows.

The mask or inhaler (*C*) is made of vulcanite, and has the shape of one half of a hollow spheroid of three inches and a half in diameter at the base and two inches and a half in depth. The rim has two notches for the prominences of chin and nose. In the centre of the mask is a hole (*d*), of the calibre of the connecting tube. The chamber above it, measuring one inch and a quarter in diameter and one inch in height, has a double fenestrated side-wall, by the turning of the outer portion of which the apertures may be either closed or opened. Through the top a short tube (*f*) enters into the chamber, projecting a quarter of an inch within it, and outside to a sufficient length to fix the connecting tube. This chamber prevents the freezing up of the end of the tube (*f*), which would otherwise sometimes happen, if it were exposed immediately to the warm breath of the patient during the administration of chloromethyl. The lateral ventilator of the chamber admits an additional supply of air if chloroform is used. At the side, corresponding to the openings of mouth and nostrils, the mask is furnished with a protector valve (*g*), to discharge the expired air.

This apparatus is very safe and convenient in its use, and a very small quantity of the anæsthetic and a shorter time are required for narcotisation. The quantity of the anæsthetic supplied is easily regulated by a gentle pressure of the bellows.

With this apparatus the anæsthetic fluid is not brought to evaporation immediately before the mouth and nostrils of the patient, as in other contrivances for narcotisation, Mr. Clover's bag excepted. With such, the vapours are inhaled in an unequally varying proportion, depending upon the quantity of fluid allowed to evaporate, and upon such diffusion in air as the ordinary mouthpieces, the porous surface of Skinner's mask, or the more or less near proximity of a saturated sponge and napkin to the patient's mouth, admit. The vapours also soon become mixed with the expired moisture, and are thus frequently reinspired. Besides, it is often beyond the power of the narcotiser to regulate the exact quantity inhaled at each inspiration. Most of these drawbacks I believe to be obviated in my

apparatus. Here continually fresh air is driven through the fluid itself, and, according to the fixed laws of diffusion, only a certain quantity of the anæsthetic can be taken up by the former. Thus impregnated, the air is brought into the mask and inhaled. The expired air passes off through the valve, and it is perfectly within the power of the narcotiser to regulate, by the pressure of the bellows, in proportion to the strength and the rhythm of the inspirations, the fresh supply of the diluted anæsthetic unvitiated by the expired air. When the bellows are at rest, the inhaler remains removed from the face, the supply of anæsthetic being stopped. This apparatus secures, besides the principal requirement—safe and speedy narcotisation—an additional advantage, that, the supply being kept in strict proportion to the demand, no loss or waste of the anæsthetic ensues from evaporation, which is often so unpleasant to those around the patient during the operation. Deep narcosis will be speedily produced by a quick working of the bellows after having accustomed the patient to the anæsthetic by a few gentle inhalations. Deep narcosis generally takes place after four to six minutes' administration of, on the average, two drachms of chloromethyl, a like quantity being required to keep up the narcosis subsequently.

Of course, the amount of vapour which passes into my inhaler varies according to the temperature of the air.

I have given chloromethyl repeatedly (since its discovery by Dr. Richardson) at surgical operations in the Samaritan Hospital and in private practice. During this time I have devised and improved my present apparatus, having at first more or less failed with the ordinary methods of administration. The first time I gave chloromethyl was in a case of vaginismus during the operation for its cure, performed by Mr. Spencer Wells on October 21. I used Skinner's mask, but, from the rapid evaporation of the anæsthetic, the patient was only, after some delay, imperfectly narcotised, and I had to resort to chloroform. I used nearly two ounces of chloromethyl within fourteen minutes, and felt the effects of the vapours myself almost too powerfully.

On the same day, during a diagnostic examination, I gave chloromethyl to one of Mr. Spencer Wells's patients, who suffered from cancer of the uterus. I used again Skinner's mask, but prevented too rapid evaporation by interposing a piece of mackintosh. It took twenty minutes before sufficient insensibility to permit examination was produced, and the patient awoke immediately afterwards. I had used fourteen drachms of chloromethyl. In the next case (see Observation *VII.*) I used my apparatus for the first time, but without the chamber. The end of the tube became plugged with ice, and I had to continue with chloroform. Since I have added the chamber I have always succeeded in producing narcosis within a short time and keeping it up during a lengthened period with a very small quantity of the anæsthetic. I have arranged twelve cases, in which I have given either anæsthetic, in the following table :—

Date.	Operation.	Operator.	Sex.	Age.	Apparatus used.	Anæsthetic used.	Time required to produce full Anæsthesia.	Quantity required to produce full Anæsthesia	Duration of Narcotisation.	Recovery after Narcotisation.	Whole time of Anæsthesia.	Whole Quantity of Anæsthetic used.	Remarks.
Oct. 22	Vaginismus	Mr. Spencer Wells.	F.	22	Skinner's mask.	Chloromethyl and chloroform	14 m.	2 ounces	..	..	..	..	Anæsthesia incomplete; continued with chloroform. No sickness afterwards.
,, 22	Diagnostic examination; cancer uteri	Do.	F.	40	Skinner's mask, covered with mackintosh	Chloromethyl	20 m.	14 drms.	20 m.	Immediate	20 m.	14 drms.	
,, 30	Ovariotomy	Do.	F.	40	My apparatus, without chamber.	Chloromethyl and chloroform	8 m.	6 drms.	33 m.	8 m.; suddenly.	41 m.	7 drachms chloromethyl; 8 drachms chloroform	Tube plugged with ice; anæsthesia continued with chloroform; sickness after nine hours; 70 minims of laudanum previously. (Observation VII.)
Nov. 6	Do.	Do.	F.	43	My apparatus, with chamber	Chloromethyl	4 m.	Less than 2 drms.	22 m.	6 m.; suddenly	28 m.	4 drachms	(Observation I.) No sickness afterwards.
,, 7	Removal of supernumerary toes	Do.	M.	9 wks.	Do.	Chloroform	2 m.	1-3rd drm.	13 m.	9 m.; gradually	22 m.	2-3rds drm.	(Observation II.)
,, 9	Vesico-vaginal fistula	Do.	F.	28	Do.	Chloromethyl	9 m.	2½ drms.	25 m.	4 m.; suddenly	29 m.	5½ drachms.	
,, 13	Ovariotomy	Do.	F.	30	Do.	Do.	6 m.	1½ drm.	19 m.	5 m.; suddenly	39 m.	4½ drachms	Remained 13 min. in deep sleep, during which no anæsthetic was given. (Obs III.) Sick five hours after operation. 50 minims of laudanum previously.
,, 19	Do.	Do.	F.	23	Do.	Do.	5 m.	1 drm. & 2-3rds	14 m.	Immediate	22 m.	3½ drachms	
,, 20	Do.	Do.	F.	23	Do.	Do.	4 m.	2 drms.	26 m.	2 m.	28 m.	5½ drachms.	(Observation IV.) No sickness.
,, 27	Do.	Do.	F.	30	Do.	Do.	6 m.	1 drm. & 1-3rd	31 m.	4 m.	35 m.	4 drachms.	(Observation V.) No sickness.
,, 28	Extirpation of a large abdominal tumour.	Do.	F.	50	Do.	Do.	6 m.	1½ drm.	19 m.	5 m.	56 m.	5 drachms	Chloromethyl given only at intervals. (Obs. VI.) Sick 15 hours after operation; 35 minims of laudanum previously.
Dec.	Ovariotomy	Do.	F.	36	Do.	Do.	5 m.	1½ drm.	12 m.	4 m.	30 m.	3 drachms	Remained 18 minutes in deep sleep, during which no chloromethyl was given. No sickness afterwards.

I subjoin several observations which Dr. E. Arendrup, of Copenhagen, kindly registered on the variations of pulse and respiration, and the duration of the narcosis both from chloromethyl and chloroform.

Observation I.—Ovariotomy was performed by Mr. Spencer Wells in the Samaritan Hospital on a patient aged 43. At the moment of commencing narcotisation with chloromethyl her temperature was 97·4, pulse 76, respiration 20. After two minutes the pulse rose to 88, and two minutes later to 108, the respiration remaining 20. At this moment (after four minutes) deep anæsthesia was completed, not quite two drachms of chloromethyl having been consumed, and the operation commenced. During the following eighteen minutes the pulse and respiration ranged as follows :—Pulse 96, respiration 18 ; pulse 100, respiration 20 ; pulse 96, respiration 16 ; pulse 100 ; pulse 88, respiration 15 ; pulse 84 ; pulse 120, quickly rising to 140, respiration 20. (This was the moment when the peritoneal cavity was sponged out, when almost momentarily slight vomiting without sickness took place.) From this moment no more methylene was given, only four fluid drachms (equal to three drachms in weight) having been used. Six minutes afterwards the operation was terminated ; pulse 96, respiration 24. She recovered suddenly and completely two minutes later, having had chloromethyl administered during twenty-two minutes, having been narcotised completely in four minutes, and under influence of the anæsthetic twenty-eight minutes. Thirty-six minutes after her recovery her temperature was 97·8, pulse 90, respiration 24. No sickness took place after the operation.

Observation II.—From a baby 9 weeks old two supernumerary toes were removed by Mr. Spencer Wells. Chloroform was given with the new apparatus. Complete narcosis took place after two minutes with one-third of a drachm of chloroform. With another third of a drachm (altogether two-thirds of a drachm) the baby was kept quiet and insensible during the whole operation, which lasted thirteen minutes.

Observation III.—A patient, aged 30, on whom Mr. Spencer Wells performed ovariotomy, had a temperature of 98·2, pulse 96, and respiration 27, immediately before the commencement of narcotisation with chloromethyl. Inhalation of one minute's duration, during which the bellows were rapidly worked, accelerated the pulse to 120 beats ; after two minutes to 136 ; after three minutes more the pulse was down to 108, when the anæsthesia was completed with a drachm and a half of the fluid, and the operation commenced. After three minutes pulse 96 ; after other three minutes, pulse 96, respiration 28. After seven minutes the administration of the anæsthetic was discontinued ; pulse 80, very strong. The patient remained in a profound sleep, breathing quietly and regularly during thirteen minutes during which no more chloromethyl was given. Three minutes after the last observation the pulse was 78, and rose after three minutes to 88, respiration 20. There were no further variations in pulse and respiration during the remaining

time of the operation. During the last three minutes some more chloromethyl was administered. She awoke five minutes afterwards quite suddenly and perfectly collected. Fifty minutes later the temperature was 98·0, pulse 84, respiration 28.

In this case a drachm and a half of chloromethyl produced deep narcosis after six minutes' inhalation, and three more drachms (four drachms and a half altogether) were sufficient to keep the patient under its full influence during thirty-nine minutes, the whole duration of the anæsthesia. Was sick five hours after inhalation, having had fifty minims of laudanum previously.

Observation IV.—In another case of ovariotomy performed by Mr. Spencer Wells in the Samaritan Hospital on a patient aged 23, chloromethyl was given. The temperature, pulse, and respiration immediately before the beginning of narcotisation were: temperature, 99·6; pulse, 100; respiration, 20. After two minutes the pulse rose to 144; after the following three minutes, pulse 120, respiration 60. One minute after the pulse sank to 128, narcosis was complete, two drachms of chloromethyl having been used, and the operation was commenced; during its progress the following variations of pulse and respiration were marked, (two drachms and a half more of chloromethyl being gradually administered). After two minutes, pulse 116, respiration 32; six minutes after, pulse 120; eight minutes after, pulse 128; four minutes after, pulse 104; two minutes after the operation was finished, and administration stopped, pulse 120. After two minutes the patient awoke immediately in full possession of her mental powers, not confused and drowsy as after administration of chloroform. Fifty minutes after, her temperature was 99·2; pulse 104; respiration 32. The whole time of narcotisation was twenty-six minutes, during which five drachms and a half of chloromethyl were consumed. The sudden rise of pulse and respiration at the commencement, and gradual fall of both during the progress of the narcotisation, are noteworthy in this case. No sickness afterwards.

Observation V.—A patient, aged 30, whose health was much impaired from the rapid growth of a large fibro-cystic colloid tumour of the left ovary, had four drachms of chloromethyl administered during thirty-one minutes, whilst ovariotomy was performed by Mr. Spencer Wells in the Samaritan Hospital. Her pulse was very small, and only 70 at the commencement of the narcotisation, but rose after three minutes to 120, respiration 27. Three minutes after, when the anæsthesia was complete, and the operation of ovariotomy commenced, the pulse had sunk again to 108, after seven minutes to 96, very small, but the respiration was accelerated to 60. After seven minutes pulse 96, stronger; after eight minutes pulse still 96, but very good; after three minutes the operation was terminated, pulse 96, respiration 30. The patient awoke perfectly composed four minutes after cessation of administration. In this case complete anæsthesia was accomplished with one drachm and two-thirds in six minutes, and kept on

twenty-five minutes longer with as little as two drachms and one-third. Also here the rapid rise of pulse and respiration, and subsequent fall of the first and its remaining stationary as regards quantity, but steady improvement in quality, are remarkable. Was not sick afterwards.

Observation VI.—A delicate lady, aged 50, whose pulse was never below 120 for several weeks before the operation, had a pulse of 142 at the commencement of the inhalation of chloromethyl, probably from nervous excitement. After six minutes she was thoroughly under the influence of the anæsthetic, of which she had one drachm and a half, and her pulse was only 104; respiration 30. The operation was commenced. After ten minutes, pulse 80, respiration 30; fourteen minutes later, pulse 100 and small, respiration 44; narcotisation stopped. After fifteen minutes, pulse 108, stronger; after seventeen minutes, pulse 100, irregular, small, respiration 36. Ten bellowsful of chloromethyl vapour. After three minutes, pulse 112, stronger; after four minutes, pulse 104, respiration 44; after seven minutes, pulse 104, very good, respiration 32. Ten bellowsful of chloromethyl vapour. After five minutes, pulse 108, respiration 36. Operation finished. Recovered well five minutes afterwards. Five minutes after recovery, pulse 120, strong, respiration 28. Slight retching after five minutes. After ten minutes, pulse 126, respiration 26. Had not been sick until fifteen hours after the operation, when she threw up about a small teacupful of mucous fluid, having had thirty-five minims of laudanum during the interval. Complete anæsthesia was produced in this patient with one drachm and a half of chloromethyl in six minutes. She remained fifty-six minutes in an uninterrupted, profound, and quiet sleep, with three drachms and a half more (five drachms altogether) of the anæsthetic, which was given only at long intervals. The operation was a very formidable one, a large cystic tumour, twenty-four pounds in weight, having been removed from the abdominal cavity, and extensive adhesions with nearly all the neighbouring intestines broken down, by Mr. Spencer Wells. The nature of the operation may have had in this case a modifying influence on the range and quality of the pulse, which somewhat differed from other observations.

Observation VII.—I add another case, in which anæsthesia was induced first by chloromethyl, and continued by chloroform. This was my first trial with my apparatus, then still incomplete. A greater quantity of both anæsthetics was used, and the end of the tube, where it joined the mask, became closed by a plug of solid ice. Such an accident is now perfectly obviated by the addition of the chamber to the inhaler, into which the end of the tube partially projects, and by the non-conducting material of which the apparatus is constructed. In this case the elastic tube was connected to a brass tube fixed to the inhaler. Also here a rapid rise of pulse and respiration took place at the commencement. It was a case of ovariotomy performed by Mr. Spencer Wells on a patient 40 years

of age. Temperature 97·6; pulse 106; respiration 22, immediately before the commencement of the narcotisation. After two minutes, pulse 144, which remained so during the next two minutes; respiration irregular. After three minutes, pulse 132; respiration 52; complete anæsthesia; operation commenced. After another minute, pulse rapidly sank to 78; respiration 40; and after two minutes, pulse 56; respiration 24. This was the moment when the tube became frozen. Two minutes later, pulse 48, when I substituted chloroform. After one minute's inhalation of the latter, pulse 78; the next minute, pulse 68. After two minutes, pulse 64; respiration 27; after two minntes, pulse 70; after five minutes, pulse 90; after three minutes, pulse 84; after three minutes more, pulse 78; respiration 20; after four minutes, pulse 76, when the operation was terminated. Patient awoke after eight minutes gradually, confused as usually after chloroform. Forty-six minutes after the termination of the operation, temperature 98·6, pulse 90, respiration 17. No sickness took place until after nine hours, forty minims of laudanum having been administered previously. In this case, seven drachms of chloromethyl were given during eleven minutes, and eight drachms of chloroform during twenty-one minutes. Complete anæsthesia was obtained after eight minutes. The whole time of administration of both anæsthetics was thirty-three minutes; the whole duration of their effects, forty-one minutes.

Chloromethyl appears to have several advantages over chloroform. Its effect takes place rapidly, and without any previous stage of excitement. No violent muscular contractions usher in the anæsthesia, but rather a cataleptic state, during which the limbs remain pliable. The patient frequently continues talking during the whole time of anæsthesia, coherently, not rambling and unintelligibly, as after chloroform. The patients awake suddenly and perfectly collected. In some cases, no sickness occurred after this narcotic; in others, it did many hours after the effects of the anæsthetic had passed off, and in all these cases laudanum had been given during the interval. In one case, sickness set in almost immediately after the operation, but could not fairly be attributed to the effects of chloromethyl, as the pathological condition of the patient fully accounted for its occurrence.

III.

COMMUNICATIONS

ON

GENERAL AND SOCIAL SUBJECTS, &c.

A NATIONAL MEANS OF INDUCING ECONOMIC SELF-PROVISION THROUGHOUT THE OPERATIVE CLASSES.

By George Cordwent, M.D., F.R.C.S.

If we thoughtfully look around us on the operative public, the features that at once take our attention, are rapid material progress, and a reckless improvidence which seems to ignore all those contingencies of life, against which, it is held, by every other class, a chief social duty to provide. Early impressions of pauperism are, I am convinced, at the root of this evil, which in the face of highly increased wages is still growing, and pregnant of the worst consequences, sapping all pride of character, and blunting the energies which develope only by self-dependence. In childhood the man has been a pauper, and by the tenacity of first impressions he in turn entails his spirit on his children. With such *morale*, then, is it surprising, nay is it not inevitable, that with higher wages there should be more drunkenness, more violence, and even more domestic poverty? For financially speaking, why need the operative make provision? why should he not loiter and dissipate his last penny, when he can readily adapt the poor-law to those temporary difficulties which a reasonable provision on his part would easily prevent or counteract?

What is more frequent than for an athletic young man earning more than £1 per week, to send for a parish medical order? The medical man attending finds perhaps the ribs broken in a drunken fight; the patient is probably an unmarried artisan, and if told that he ought not to require parochial relief, he confidently refers to his "order," and says "My ribs are broken, I cannot lie here and die, I have no money and no one will trust me." Of course such an argument is conclusive at the time, and the man must be administered to by medical attendance and parochial money. But why, I ask, should an unmarried man be allowed with impunity, thus recklessly to neglect self-provision, when a married man with even less income is held penal if he neglect to provide for his wife and family, and so allow them to become a burthen on others?

Let us suppose another frequent case, a domestic servant leaves her place after a service of some years, at perhaps £15 per year; she suffers a temporary illness, and instantly applies for a paro-

chial order; you will say this is prudent, or at least only venial, and so I should quite take it to be if the matter ended here; but look at the evil it entails. Did a person ever receive one charity, and not apply with less compunction for a second? Did a person ever get medical attendance on the parish account in a first illness, who was not certain to apply for bread and money in the second? And experience fully teaches us that a father or a mother who has leaned in single life on public alms, is certain on the slightest pressure to bring up a pauper family.

How is the evil to be remedied? That is the subject of this paper. Now if it be right, as it undoubtedly is, that a married man should be held responsible for the maintenance of his wife and children whilst the latter are still incapable by youth of maintaining themselves, it surely is no less just to compel the unmarried adult to secure himself and society against his being liable, on the slightest reverse, to fall back on public or private alms. It is true, however, that the *practice* of the poor-law is so entirely understood by the operative classes, and so worked by various expedients to their pecuniary assistance, that few indeed having families fail "by hook or by crook" to press a poor-law contribution into their service. The illness of a child, more frequently feigned than real, is a ready condition of relief perfectly understood, at least in country places, by all the parties concerned; and it is clear that this relief is often an expedient for counteracting many repulsive hardnesses which exist in the original structure of the law. Still, let it be seen that the aggregate of such relief is a vast permanent expenditure, over which the scheme herein proposed would give but a temporary and slight increase; so temporary that it would be from the first diminishing. But if the law distinctly declares that every able-bodied man shall provide for his non-adult children, however numerous, surely it should require every healthy adult not possessed of private property to make a self-provision sufficient to guard him against the ordinary chances of requiring public alms. By the term self-provision, it is here intended to express a regularly repeated monetary deposit in a state security, and bearing the best interest the state could allow.

A deposit of about 2d. per week commenced at the age of 13, and economised at interest, would secure to the individual 3s. per week during illness, and a permanent allowance of 2s. 6d. a week after 65 years of age. Less than 2d. per week more would pay even liberally for medical attendance, nursing, extra dieting, and burial expenses. The latter should from the first be at the cost of the assured, by a fractional increase of that weekly deposit which may assure the sick and superannuation pay. But the expenses of medical attendance, of extra dieting, etc., and of all poor-law salaries, and centralised machinery, should be paid during a time from the poor-rate, collected as now, till the enormous contingent fund resulting from the lessened numbers dependent on the rate could exactly regulate its levy. By and by when the habit of self-dependence and

resulting self-respect have been engendered, the expenses at first paid by the fostering aid of the poor-rate would, I am convinced, be voluntarily assumed by those at first parochially aided. The premises herein assumed I trust are worth dilating on, for if correct, the great bane of this country, its over grown, its business-like pauperism, can be leniently and step by step corrected. Wages are now sufficient for pressure of a measure such as that here proposed, they ought to be sufficient, they will soon be much more than sufficient, for there are strong indications that the terms of the labour-market will rise, though science and national intercommunication will probably much reduce the price of provisions. Let, therefore, legally enforced self-provision be earnestly considered.

But with the evident ability of multitudes of operatives to make future provision for themselves in the way indicated, it is equally clear that the majority of middle-aged labourers having several young children could not make such provision for themselves and families; that difficulty must be met by the revenue of the poor-rate.

The working of the scheme proposed in this paper would begin with the *nominal* supposition, that every parent is capable of making the contingent provision for himself and children under the age at which they may become personally responsible for self-provision. And the official demand being made, not only on operatives, but on every one not paying income tax, a right of appeal should be allowed, and as now assessed taxes may be appealed against, so each Board of Guardians should hold annual appeal days. The appellants would of course be of three classes. Those having property above the scale which would determine exemption from poor-law interference. A second class would be partially or entirely paid for out of the rate, according to age, health, wages, and the pressure of their families. It must here be observed, that the assurance or provisional deposits would be lighter in proportion as they applied towards infancy, and to that extent, would favour the youth assuming payment, and be to him a valuable inducement. The third class would be the younger adults, single or having small families. And after even a lenient hearing, there would be discovered a very large proportion amply capable of paying for themselves in the manner proposed, but who now spend a surplus income in the worst forms of pleasure, and taste thereby the very dregs of poverty.

There is, however, an obvious cause of serious sectional poverty, not attributable to improvidence, but to temporary depressions in especial branches of industry, and to this cause no doubt the strong inducement to and facility of emigration would bring even too ready a corrective. But with the present unthrifty and tenacious disposition of the operative classes, a failing of employment ever so *temporary*, paralyses whole masses by a poverty so complete and starving, that it must be at once administered to. And let it be administered to, but certainly not by organized alms and parochial re-

lief; these make more inveterate an element already rife to repletion; but by Government using a portion of its now vastly increased poor-rate revenue to employ such sections of operatives in productive works at fair average wages. Let any refusing them be afforded residence in the different workhouses, and failing this also, let them be considered impostors and treated accordingly. This mode of living by a temporarily substituted branch of industry would bring no loss of personal respect to the operative, then receiving only for what he contributed, instead of, as now in such a case, being maintained by alms until sloth and chronic mendicity become engendered. If the wages paid under such circumstances by Government were somewhat lower than those received by the artizan in prosperous times, it would be reasonable, and perhaps wholesome; for let necessity teach prudence, as it is intended to teach every class, and by its temporary corrections liberally compensate, except to the debased, in new impulse and circumspection.

I come now to the especial, and what many think (I believe too hastily) an insurmountable, difficulty—the collection of the weekly investments. We see Government just now commencing an almost parallel experiment in collecting poor-rate from the very poorest cottager, and I venture to say there will be no great difficulty. We see also that some of the worst paid farm-labourers have for more than two generations voluntarily taxed themselves with contributions to the crude, the illusory assurance of Friendly Societies, with all their contingent outlay of public-house meetings; and we see, alas, that their ill-devised systems bring inevitable failure, and bear only blight when harvest should be ripening. In fact, the contribution to such societies not being based on valid calculations, they all die out insolvent, and yield the poor man ultimate regret, instead of the fruits of a long cherished hope. Still, they contribute, and I can see no difficulty in an organized collection for a far better and reliable purpose.

Officers like relieving officers must collect, and report defaulters to the Board of Guardians, to whom or to magistrates must be given summary and specific jurisdiction in the matter; such officers would also have to pay the sick and superannuated, and distribute any especial articles of dietary ordered by the medical officer. They should also keep an *individual* account for or with every compelled insurer in their district, so that at any time could be ascertained the exact collective sum each individual has paid; and in case of an *enfranchisement*, to be more particularly referred to, that sum could be paid over to some other safe investment, in future more private and agreeable to the enfranchised person. Still, however, to guard against a relapse into pauperism, the poor-law authority should be a permanent trustee, and the fund therefore not negotiable without concurrence. So again in the event of emigration there may be returned such part of the collective sum paid by the assured emigrant, and in such time and portions, as reason and experience may justify.

Both the medical officer and the relieving officer, if he may be so named, should be elected by the people who would have the chief interest in their election, but once appointed, there should be no means of discharging them except such as now exist, and so no destructive working for popularity.

By time and accidental circumstances the improved condition of many would make unnecessary and offensive a weekly supervision by Boards of Guardians, so any who could show that they possessed £30 besides sufficient household goods, or such other sum or possession as in the wisdom of Government may be thought sufficient to provide them against the chances of pauperism, should be allowed to claim enfranchisement, and, under suitable authoritative precaution, transfer to some other safe investment the sums that pertain to them. To take a less classified, and therefore less offensive view, let every one not paying income tax prove his enfranchisement, and such, removing him from the stigma and base influence of alms, would I think be a safer claim to political franchise than that which at present bestows it; many would soon claim this enfranchisement, and so establish an unequivocal position.

From the age of 13 to 35 there is comparatively little illness, so many would postpone coming under the law, or altogether avoid coming under it; but of course the later a person began his investment the more he would have to pay weekly, and moreover in case of sickness, or other cause of necessity, he would be legally punishable for not being able to show such self-provision, when falling under poor-law administration. The same would apply relatively to any one who being enfranchised brought his capital below par; nothing could be legally done till he applied for relief, when he must reassure at the age of this occurrence; and herein lies, not only a strong inducement to economy, and to keep up an assurance commenced in youth, but also an essential point for the preservation of some authoritative guardianship over enfranchised funds.

Much astonishment is expressed in periodicals that poor-rate expenditure does not rise and fall, as it ought to do, in something like true proportion to the rate of wages and price of provision, but it is seen that these have only a slight and certainly disproportionate influence. The cause is obviously what is here stated: the facility with which the poor-law can be made to substitute self-provision; and no matter what the rate of wages, there will be excess of pauperism until you *enforce* a provision for themselves on the part of the younger adults.

An extensive correspondence on this subject teems with evidence that unmeasured extravagance, not necessity, causes the enormous draw on our poor-rate, and men of experience everywhere think it high time that a little pressure should be made in a new direction. Let a man first so contribute as to be independent of public alms, and then let him squander if he please. There is nothing un-English nor oppressive in a law to restrain trespass against the property of

others, and if it be constitutional to compel vaccination as a means of preventing infection, it is equally constitutional to enforce a lenient provision against the imminent contingencies of life, and so prevent the unfair shifting of a natural burden. For surely there is no tyranny in obliging a man to maintain himself when able to do so, but there is *much tyranny* in obliging another to maintain him. Were such enforced provision to become law, a few years would bring a vast reduction in the poor-rate expenditure, and, what is of much more importance, this reduction would concurrently elevate from a facile pauperism which taints even by short participation. I do not say there would be saving from the first, nor believe it, because of the continuous weekly investments which would have to be made from the poor-rate for those who could not make such for themselves; but in the absence of statistics by which such increase may be calculated, I believe it would not be great, and by its nature be from the first diminishing. I trust it is clear that abrupt withdrawal of poor-rate assistance is not advocated in this paper, but that under such assistance there should be an easy, but rigidly enforced transition from pauperism to personal investment; and this scheme is thrown out in its crudity, that it may receive from others the practical judgment I am convinced it is worth.

It will be observed that great disproportion would soon occur in the relative rating required in different parts of England, so that whilst in some mercantile places the poor-rate may perhaps be relieved by one-third in five years, in many of the western parts of England perhaps it could be relieved by but one-tenth in that time. Whether low wages should be left retributive in the unions where such are paid, or all poor-rate collections be centralised in a national fund, I will not pretend to suggest; but a general current tends towards centralisation, and it can scarcely be otherwise in a country like this, where interests so rapidly and closely communicate. But whether the accounts were still local, or more nationally centralised, and local only to the extent of simple administration, there would under either mode be a favourable result. And if the maximum expense at the onset were greater than that under the present system, the decrease would by its nature be from the first, and on a scale which soon would more than compensate for the weekly assistance in contributions which those having young children would require, and we should at length be in the path to check a chronic pauperism which has ramified into the very core of society.

IV.

ACCOUNT OF THINGS EXHIBITED.

Dr. Crisp exhibited a large number of wet preparations, casts in wax, and drawings of various forms of tubercle in man, quadrupeds, birds and reptiles, taken from animals he had dissected; including verminous tubercle in the sheep, pig, and rabbit. The casts were taken first with plaster of Paris, then in wax, and coloured after nature.

Dr. Crisp also exhibited the eyes of 600 different species of vertebrate animals, mammals, birds, reptiles, and fishes, which had been at the late Paris Exhibition. They were prepared by the exhibitor in the following novel manner, for the purpose of showing the colour of the iris, and the relative size of the eye to the body. The contents of the eye were removed, and the cavity filled with plaster of Paris; when dry the eye was painted in oil colours, so as to imitate the natural appearance. Among the specimens were the eyes of man, of the human fœtus at various ages, of the gorilla, orang, chimpanzee, white bear, lion, tiger, chetah, seal, whale, Tasmanian wolf, koala, wombat, orycteropus, ant-eater, ornithorhyncus, echidna, giraffe, koodoo, elk, elephant, hippopotamus, rhinoceros, tapir, wart-hog, hyrax, eagle, vulture, great owl, ostrich, rhea, cassowary, mooruk, brush turkey, alligators, crocodiles, turtles, tortoises, monitor, lizard, puff-adder, cobra, rattle snake, tunny, moon-fish, sword-fish, etc., etc.

Dr. Mackinder exhibited a calculus about the size of a goose's egg, which he had removed by lithotrity from the bladder of a female aged 23, who had suffered many years; recovery.—A mulberry calculus removed by lithotomy from a cyst in the bladder of a boy, aged 9; recovery.—Uric acid calculus, weighing two ounces, which he removed by the Median operation from a man aged 64, who had been suffering eight years; recovery.—Phosphatic calculus weighing five drachms, which he extracted by the Median operation from a man aged 75. He died on the fourteenth day, of pneumonia. The post-mortem examination revealed a gall bladder full of gall stones. This man had previously been under Sir W. Fergusson, who had performed lithotrity, but so great was his suffering then and afterwards that he refused to undergo the same operation again.—A triple phosphate calculus, weighing four ounces and one drachm, which he removed by Median lithotomy from a man aged 38; recovery. Probably the largest removed by that operation.—A tubular calculus,

alias three inches of tobacco pipe, removed from the female bladder; recovery.—An enormous gall stone, which fell through an opening in the integuments of the right inguinal region of an old lady who had suffered from much abdominal pain and recurrent jaundice for about two years; recovery.—A long, tail-like collection of hair, about an inch and a quarter in diameter, passed from the bowels of a young dressmaker, who had suffered from enteric irritation for several years. She had the habit of swallowing the hairs which she plucked from her head; recovery.—Two kidneys of an old woman, entirely destroyed by hydatids, the urine having passed by the bowels for many years. She never would take medicine for her *diarrhœa*, but said she might be opened when she was dead. Many cysts were also found in her liver.—The ruptured bladder of a man, the immediate cause being the stoppage of a false passage in the urethra by a calculus about the size of a millet seed.—A case of instruments for the treatment of fistula in ano according to the plan proposed in his paper in an earlier part of the present volume.

Dr. Sedgwick exhibited sublimates of strychnine, morphine, atropine, salicine, santonine, picrotoxine, and arsenious acid; and sulphates of iodo-morphine, iodo-codeine, iodo-atropine, and iodo-strychnine.

V.

APPENDIX.

NOTE.

It has been thought advisable by the Council, to add as an appendix an abstract of the proceedings of the General Session held on April 8th, 1868. The detailed account will be given in the next volume of Transactions, but in order that the members of the Association may be made acquainted with all that has been done up to this time, the Honorary Secretary's report on Parliamentary matters, and the statement drawn up in regard to the opposition of the General Council of the University of St. Andrews to the admission of the Doctors of Medicine to a seat in that Council, have been published in full.

April 20th, 1868.

REPORT ON PARLIAMENTARY MATTERS.

By Leonard W. Sedgwick, M.D., Hon. Sec.

The Report of the Council presented to the members of the Association at the Anniversary Session held in December last contained a narrative of the steps taken to place before the Government, the University authorities, and the Profession, the claim of the Medical Graduates of St. Andrews to a vote for the Member in Parliament for the conjoined Universities of Edinburgh and St. Andrews. The object of this report is to extend that narrative to the present time, and to state as succinctly as possible the actual position of the question.

At the Anniversary Session it was resolved that the Lord Advocate, and if necessary, the Right Hon. B. Disraeli, should be asked to receive a deputation from the Association on the matters affecting the Medical Graduates of St. Andrews contained in the Scotch Reform Bill.

Through the intervention of Dr. Macintyre, Mr. Sclater Booth, M.P., obtained for the Council an interview with the Lord Advocate. The notice of the time fixed was so short that it was impossible to communicate with more than a few members of Council living in London, but the President, Dr. Rowdon, Dr. Wynn Williams, and myself were received by the Lord Advocate on December 6th, 1867. He gave a very careful and courteous attention to the statements laid before him, and he promised to give them his earnest consideration, and to consult with his colleagues as to the course to be pursued. An objection to the claim, which seemed to have considerable weight with him, was, that as upwards of 600 candidates for the degree passed in 1862, the examination could not be considered a satisfactory one. In order that an authentic statement of the facts should be laid before him, I wrote to the Registrar of the University asking for details, and received the following reply :—

"University of St. Andrews.
"December 9th, 1867.

"Dear Sir,

"In reply to your letter of the 6th inst. in reference to a return "of the St. Andrews Medical Graduates ordered by and transmitted "to the House of Commons, I beg to inform you that in 1862, 606 "persons obtained the degree of M.D.

"The large number in that year is accounted for from the cir- "cumstance that, on January 1st, 1863, the new regulations, by

"ordinance of the University Commissioners, came into operation, "restricting, in one section, the number of graduates in medicine of "St. Andrews to ten annually.

"A return of the members of General Council, recently trans- "mitted to the House of Commons, was printed and made public; "and the return to which you refer, will, I presume, be also "printed.

"I am, dear Sir, yours truly,

"WILLIAM TROUP, *Registrar.*

"Dr. Leonard W. Sedgwick.

"P.S.—In 1862, 86 candidates were, after examination, remitted "to their studies; of this number 24 subsequently passed, and 62 "were finally rejected. Two candidates withdrew. In 1861, 107 "candidates graduated, and in that year 14 candidates were "finally rejected.—W. T."

On receiving this I wrote to the Lord Advocate, enclosing an extract from that part of the letter relating to the year 1862, and adding:—

"I venture to suggest that the large number of candidates, 694 "apparently, shows that the number ten, to which after 1862 non- "residents are restricted, does not meet the wants of the profession; "and that the number which, under ordinary circumstances, would "have been distributed over many years was, because it was "practically the last chance, concentrated on this year.

"I would also ask permission to direct your attention to the large "number of primary rejections, 1 in about $8\frac{1}{2}$, as evidence con- "clusive of the fallacy of the belief that there was any laxity of "examinational test, especially when it is remembered that all "the candidates were at the time of their examination duly qualified "practitioners, or had complied with the curriculum of the "University."

This was written on December 10th, 1867.

At a meeting of the Council, held on January 16th, 1868, I was instructed to ask for the decision of the Lord Advocate in the matter, and accordingly I wrote to him as follows:—

"2, Gloucester Terrace, Hyde Park,

"January 18th, 1868.

"My Lord Advocate,

"I am instructed by the Council of the St. Andrews Medical "Graduates' Association respectfully to ask for your Lordship's "decision in regard to the claim of the Medical Graduates of St. "Andrews to a vote for the Member in Parliament for the con- "joined Universities.

"The Council are so acutely sensible of the injustice of clause 30 "of the late Bill that they would feel it their duty (in the event of "your Lordship's decision being adverse to them) very urgently to "press for an interview with the Chancellor of the Exchequer, in

"order that they might lay before him the nature of their claim, "the great injury to the professional status and material prosperity "of a large body of educated men which the proposed exclusion "would effect, and the identity of their relationship to their Univer- "sity with that of the Graduates of the University of London to "theirs, who by the Reform Act had the franchise conferred on "them.

"Under these circumstances I venture to ask that your Lordship "will very kindly make me acquainted with your decision during "the ensuing week.

"I have the honour to be,

"Your Lordship's obedient Servant,

"LEONARD W. SEDGWICK, M.D.,

"*Hon. Sec. St. And. Med. Grad. Ass.*

"The Right Hon. the Lord Advocate."

His reply was this :—

"Edinburgh, January 23rd, 1868.

"Dear Sir,

"In answer to your letter regarding the St. Andrews Medical "Graduates, I have to state that the determination of the question "whether they are to form part of the University council and "constituency lies with the Cabinet, and I am not at liberty to "express any opinion upon the matter.

"You should, therefore, take such course as appears most "expedient.

"Faithfully yours,

"EDWARD T. GORDON.

"Dr. Leonard W. Sedgwick."

It thus became necessary to appeal to the higher powers, and as Sir Graham Montgomery, Bart., M.P., had promised last autumn to procure for the Council an interview with the Chancellor of the Exchequer, I wrote to him asking him to introduce a deputation. He replied :—

"Stobo Castle, January 29th, 1868.

"Dear Sir,

"I have received your letter of the 23rd, As I am at present in "Scotland and not intending to be in town until just before the "meeting of Parliament, I think it would save time if you were to "address a letter on behalf of the St. Andrews Medical Graduates' "Association to Mr. Corry, Downing-street, the Chancellor of the "Exchequer's private Secretary, and ask for an interview, stating "your reasons therefor. Had I been in town I could have helped "well enough, but in the circumstances I think it would be better "you applied directly as I have suggested.

"I am, yours faithfully,

"G. GRAHAM MONTGOMERY.

"L. W. Sedgwick, Esq., M.D."

Acting on this suggestion, I wrote to Mr. Corry asking for an interview of the Council with the Chancellor of the Exchequer, and enclosing a copy of the report of the Council and my paper on the University of St. Andrews and its Medical Graduates, to which he was good enough to reply :—

"Downing Street, February 7th, 1868.

"Sir,

"I have laid before the Chancellor of the Exchequer your letter "to me of the 30th ult., and the report of the Council of the St. "Andrews Medical Graduates' Association, which you also sent me. "That report and the remarks by yourself appended thereto, "contain such ample information as to the position of the medical "graduates of St. Andrews with reference to the franchise, that the "Chancellor of the Exchequer doubts whether any considerable "advantage would be gained by the interview which you propose.

"He would, however, have had much pleasure in receiving the "Council of the Association according to their wish, but for the fact "that he has found it absolutely necessary to make it a rule, to "refuse all deputations on matters of detail connected with Reform "—a rule which during last session he invariably adhered to. In in- "forming the Council of the reason why the Chancellor of the "Exchequer cannot comply with their request, will you be so good "as to assure them that he will give his most careful considera- "tion to the facts and arguments which you have laid before him, "as well as to any other statement, in addition, which you may "think proper to send him.

"I have the honour to be, Sir,

"Your obedient Servant,

"Montagu Corry.

"Dr. Sedgwick."

This seemed so favourable an opportunity of gathering together into one consecutive statement the different facts concerning the requirements of the University, the mode of examination, and the social and professional position of its medical graduates, as well as the replies to the various objections which had been urged against their admission to the franchise, that with the sanction and approval of the President I drew up the following memorandum, and forwarded it to Mr. Corry, along with this letter :—

"2, Gloucester Terrace, Hyde Park,

"February 12th, 1868.

"Sir,

"I have the honour to enclose an additional statement con- "cerning the claims of the medical graduates of St. Andrews to "the franchise of the conjoined Universities.

"Every objection of which I am cognisant is there replied to, "with the exception of one,—that the St. Andrews graduates if "admitted would 'swamp' the Edinburgh graduates. But as it "is impossible that this has been seriously advanced, it is not

"there referred to. The St. Andrews graduates, as a matter of "fact, are not so numerous as the Edinburgh graduates, and if "it were not so, their number would be an aggravation of the "injustice if their claim were good, but no argument in their favour "if it were bad.

"I venture to ask that you will kindly inform me if any other "objection has been brought to the notice of the Chancellor of "the Exchequer, in order that it may be replied to; and also that "you will be good enough to give me early intimation of the "decision of the Cabinet in this matter. The vital importance of "the question to 1300 Doctors of Medicine will, I am sure, be "deemed sufficient excuse for the urgency of this application.

"I am, Sir, your obedient Servant,

"LEONARD W. SEDGWICK, M.D.

"Montagu Corry, Esq."

MEMORANDUM concerning the claims of the St. Andrews Medical Graduates to the Franchise, submitted for the consideration of the RIGHT HONBLE. THE CHANCELLOR OF THE EXCHEQUER.

"THE regulations regarding the conferring of Medical Degrees in "the University of St. Andrews adopted in 1826, continued in "force (with only slight alterations in 1833) until 1863, when they "were altered by the Scotch University Commissioners.

"The Degree of Doctor of Medicine of St. Andrews is recognised "by 'THE GENERAL COUNCIL OF MEDICAL EDUCATION AND "REGISTRATION OF THE UNITED KINGDOM,' and entitles its holder "to the same status and confers on him the same legal rights as the "Degree of M.D. of any other University.

"The regulations empowered the University to confer the Degree "of M.D. on any legally qualified practitioner of good character "after passing the requisite examination. In 1863 the Scotch "University Commissioners continued this power, but restricted the "number of Degrees granted in such manner to ten annually, and "required the candidates to be upwards of forty years of age.

"The regulations in force both before and after 1863 provide a "curriculum of the studies to be pursued in the University by "resident students who are desirous of obtaining the Degree "of Doctor of Medicine; but as there is no recognised hospital "at St. Andrews and no means of obtaining a thorough medical "education there, these regulations have been practically in "abeyance.

"During the war early in this century the Degree of M.D. was "conferred on persons obtaining medical appointments for instant "duty in the army and navy, on the certificate of eminent London "physicians or surgeons, but since then no one has been admitted "except after examination.

"The Examination for the Degree lasts three days, is partly "written, partly *vivâ voce*, and is in every respect similar to that "for the Membership of the Royal College of Physicians of London.

"The Examiners are unknown to the candidates, and a certain "number of them are always distinguished Graduates of other "Universities.

"The Scotch Reform Bill of last session provided for the admis- "sion to the General Council of their University, and in consequence "to a vote for its Member in Parliament, of those Graduates only "who had attended classes during one Session in the University: "with the exception, in the case of St. Andrews, of those Doctors of "Medicine who had obtained their Degrees since 1863.

"By this clause, all the Doctors of Medicine of Edinburgh would "be entitled to be placed on the General Council and to vote for the "Member of Parliament; whilst all but 46 out of the 1350 "Doctors of Medicine of St. Andrews would be excluded both "from the General Council and the franchise. The 46 voters are "admitted under the proviso; they have not resided at the Uni- "versity, and their examination has not been more stringent, but "has been less in extent than that of the Graduates who passed in "previous years.

"The reasons which have been given for the exclusion from the "franchise of their University of a large body of educated men in "positions of responsibility and trust are various. It has been "said—

"(1) That the Degrees have been sold. The Registrar of the "University writes that there is no truth in this statement.

"(2) That the examination is poor and unsatisfactory. For some "years before 1858 the rejections were in the proportion of 1 to 4 "admissions. No calculation has been made since then until 1862, "when 606 candidates passed; in this year the rejections were 1 in "8½. This large number of admissions is no proof of a lax exami- "nation, but furnishes strong evidence that the limitation in the "number of M.D. degrees granted by the University to ten "annually, and to candidates above 40 years of age, does not "meet the requirements of the profession and the public. Very "many do not wish to become Doctors of Medicine until they "have been some years in practice and are able to know whether "such a degree would be of advantage to them or not. Such men, "and those who for various reasons—improving social position, "a wish to become consultants, &c.,—desired, or thought that at "some future time they might need, the M.D. degree, and who under "the old rules would have been distributed over many years, were "in consequence of the new regulations obliged to present them- "selves for examination in this year, or be excluded in large numbers "by the limitation to ten annually. Under these circumstances, and "considering that all the candidates were at the time of their ex- "amination legally qualified practitioners, the number of rejections

"(1 in $8\frac{1}{2}$) is conclusive as to the validity of the examinational test. "The Army and Navy Medical Boards require every candidate for "employment in the services to be at the time of his application a "legally qualified man and to undergo another examination: 32 "per cent of the candidates who are Members of the Royal College "of Surgeons of England, 18 per cent of those who are Doctors "of Medicine of Edinburgh, and only 8 per cent of those who are "Doctors of Medicine of St. Andrews, fail to pass. In London, 39 "Doctors of Medicine of St. Andrews are attached to the Hospitals, "the Medical Schools, and the large Infirmaries; including among "them St. Bartholomew's, Guy's, St. George's, Westminster, London, "Charing Cross, Bethlehem, St. Luke's, and 22 similar institutions. "*In the provinces, there is hardly a large town in which a Doctor of "Medicine of St. Andrews does not hold an important post in connec-"tion with the chief Hospital.* Nineteen Medical Superintendents of "County Asylums are St. Andrews M.D.'s; as also two Deputy-"Lieutenants, twelve Justices of the Peace, ten Coroners and "Deputy-Coroners, six Examiners in Medicine, six Officers of Health, "three F.R.S., eleven F.R.C.P. Lond., and fifty-five F.R.C.S. Eng. "The ratio of rejections and the success of the Doctors of Medicine "of St. Andrews at the Army and Navy Boards are proofs of the "strictness of the examination, and their social and professional "position is cogent evidence of public confidence and esteem.

"(3) That the majority are Englishmen, and should not vote for "a Scotch University. If the University does not refuse to admit "to examination any one who is not a Scotchman and to grant him "the Degree of M.D., there can be no reason (in the absence of "express statute to the contrary, in existence prior to the candi-"date's offering himself for examination) why he should be "deprived of the rights and privileges which in other Universities "pertain to that degree. In no other University, and now not in "St. Andrews, is nationality taken into account, and by the late "bill no English graduate of Edinburgh was excluded from the "franchise of his University.

"(4) That the Medical Graduates have not resided in the Uni-"versity. If St. Andrews is to grant Medical Degrees at all it must "grant them to non-residents, for it has no Hospital and no School "of Medicine. And even if it were in future to require residence of "all its Graduates, those who have already obtained their Degrees "should not be affected by regulations which were not in existence "at the time of their examination. The Doctors of Medicine have "complied with all the regulations of the University, and claim to "be entitled to exercise every right or privilege which may be "granted to any Medical Graduate. The University of London is "in a similar position, it provides no Medical School, and consequently "requires no residence within its walls; but it has a representative "in Parliament, and its Doctors of Medicine have a vote in his "election.

"Should the Medical Graduates of St. Andrews be excluded from "the franchise the Doctors of Medicine of the United Kingdom "would be divided into two classes, voters and non-voters, the "latter being nearly all the St. Andrews men. The result would "not be doubtful. The public would recognise this difference as a "mark of inferior rank and knowledge; and a large number of "educated men, scattered over the whole kingdom and possessing the "confidence and esteem of their neighbours, would be exposed to "injurious comparison with their brethren, who possessing no "better degree are yet voters; and would suffer a grievous injustice "and a serious injury to their social and professional status; an "injury which, in the army and navy, would not be confined to "the individual, but would be destructive of the subordination and "efficiency of the services.

"February 12th, 1868."

Having done all that was at this time thought advisable in bringing our case before the notice of the Government, it was felt that the cause might be materially strengthened if the support of certain Members of the House of Commons, whose opinions on such a matter would have great weight, could be obtained. Among these Members were notably, Mr. Stuart Mill, the rector of the University; Sir W. Stirling Maxwell, Bart., one of the late Scotch University Commissioners; and Mr. Ellice, the member for the city of St. Andrews.

In reply to a letter in which I enclosed the memorandum, Mr. Stuart Mill wrote:—

"Blackheath Park, Kent.
"February 15th, 1868.

"Dear Sir,

"The St. Andrews Medical Graduates seem to have a strong "case. I shall endeavour to obtain for them a fair consideration "when the Scotch Reform Bill is before the House.

"I am, yours very faithfully,
"J. S. Mill.

"Dr. Sedgwick."

Having obtained an introduction to Sir W. Stirling Maxwell, Dr. Macintyre (to whose zeal and energy and influence so much of the success which has hitherto attended the efforts of the Association in its vindication of the rights of the medical graduates of St. Andrews is due), came up from Odiham, and, accompanied by the President, called upon him. Sir W. Stirling Maxwell was from home, but he was good enough subsequently to write to the President and make an appointment with him. On February 18th, the President and myself went to the House of Commons, where we saw Sir W. S. Maxwell. The President entered very fully into the subject with him, and all the arguments for and against our claim were carefully considered; at the end of a long conversation we had

the satisfaction of knowing that Sir W. S. Maxwell thought our case unanswerable. The Lord Advocate and Mr. Ellice were also seen, and the latter promised to support us in the House. Lord Campbell, too, offered his assistance in the House of Lords.

The matter having thus been laid before the Government, and the more influential members of Parliament connected with the University of St Andrews, it was hoped by the Council that a consideration of the facts would have its due influence. In this they were not mistaken, for on the 21st of February the Scotch Reform Bill was in print, with these clauses relating to the University franchise; clauses which, if carried, will secure to the Graduates in Medicine the same rights and privileges as the Graduates in other Faculties, and the same rights and privileges as the Doctors of Medicine of other Universities.

"26.—Every person whose Name is for the Time being on the "Register of the General Council of any One of the Universities of "Scotland shall, if of full Age, and not subject to any legal Incapa- "city, be entitled to vote in the Election of a Member to serve in "any future Parliament for such University in Terms of this Act. "Provided that no Person shall be entitled to vote at such Election "unless he shall have paid, on or before the Day on which the "Writ for the Election is proclaimed, all Fees due by him as a "Member of the said General Council as at the Date of such "Proclamation.

"27.—Besides the Persons constituting the General Council of "each of the said Universities in virtue of the Sixth Section of the "Act 21st and 22nd Victoria, chap. 83, every Person shall "be entitled to be a Member of any such General Council on "whom the University has after Examination conferred the Degree "of Doctor of Medicine, or Doctor of Science, or Bachelor of "Divinity, or Bachelor of Laws, or Bachelor of Medicine and "Master in Surgery, and the Name of every such Person shall "be registered in the Book kept for the Purpose, on Payment of "the usual annual or other Fee."

A subsequent clause, "32," containing the regulations to be observed with respect to the polling, makes this no barren privilege, for it provides for the issue of voting letters, and thus enables every member of the General Council, wherever resident, to take part in the election of the Member in Parliament for his University.

But this report would not be complete without a reference to the action taken by the Universities of Edinburgh and St. Andrews.

In Edinburgh we have strong opponents, and strange to say, in members of our own profession; but we have supporters too. The University Court, at a meeting composed of Drs. Alexander Wood and Christison, Mr. Macknight, Mr. Laing, and the Rev. — Phin, were of opinion that the admission of the Doctors of Medicine of St. Andrews to the General Council and the franchise of their

University, would seriously compromise the interests of the University of Edinburgh in the event of its being united with the University of St. Andrews in returning a member to Parliament, and resolved to bring the whole subject under the notice of the Government. But the Senatus Academicus, a body which comprises the Principal and the Professors of the University, at a meeting held after that of the University Court, resolved unanimously—"That the Senatus Academicus, having had their "attention directed to the decision of the University Court to oppose "the proposition that Graduates in Medicine of the University of "St. Andrews should receive electoral privileges under the Scotch "Reform Bill, resolve not to join in the opposition."

Our hearty thanks are due to those enlightened men who discarding the narrow and ignoble plea of separate interests, have seen that there could be no real antagonism between the Doctors of Medicine of St. Andrews and the Graduates of Edinburgh, and who have refused to range themselves in opposition to those with whom they may in a short time be associated as fellow-voters, and who are members of the same profession as many of themselves.

And what says our own University to this matter? Surely it is foremost in the fight, urging on the authorities with all the energy of hearty conviction and the weight of a learned and ancient body, the just claims of the most numerous section of its Graduates, by whom and through whom, it may with the barest truth be said, it lives and moves and has its being.

Alas, no! The University Court has not spoken. The Senatus Academicus, to whom in the trustful confidence of youth we turned for encouragement at our first meeting, and for whose "generous sympathy and active support" we asked in June last, have as yet uttered no sound. But the General Council has met; that body, composed of 368 Graduates in Arts, in whose hands up to the present time the academical interests of the 1300 Doctors of Medicine have been placed, has been pleased to consider our position. Not many of the 368 have spoken, only some 23 were there, and this is what they have done. They have resolved to petition Parliament against the clause in the Reform Bill for Scotland which provides that Graduates who have not given residence at the University be made members of the University Council. It was not without opposition that the resolution was passed. Professors Macdonald and Bell spoke out manfully for their brethren, but they were alone. And now the General Council of the University of St. Andrews is committed to the position that it is right and fair to grant to successful candidates the Degree of M.D. after an extended examination, and to take their fees, and yet to refuse to acknowledge the holders of such Degrees as Graduates of the University by the only recognition worthy of educated men, that of a voice in its Councils.

Postscript.—In regard to this last matter, it was proposed by Dr. Cholmeley, seconded by Dr. Ross, and carried unanimously—

"That the Honorary Secretary be directed to draw up a statement "showing the grounds on which the Doctors of Medicine of St. "Andrews claim a right to be placed on the General Council of the "University, and refuting the arguments raised against their "admission; and that he be also directed to forward such statement "to the Governing Bodies of the University, with an expression of "the deep regret of this Association at the determination of the "General Council to oppose the claim of the Doctors of Medicine, "and of their hope that this opposition will not be persevered in."

In accordance with this resolution, the following statement has been issued.

MEMORANDUM concerning the claims of the Doctors of Medicine of St. Andrews to be Members of the General Council of the University.

The "Representation of the People Bill (Scotland)" now before the House of Commons, provides that the Universities of Edinburgh and St. Andrews shall jointly return one Member to serve in Parliament; it declares that the Members of the General Council of each University shall be the Voters; and it enlarges the constituency by admitting to the General Council "every Person on whom the "University has after Examination conferred the Degree of Doctor "of Medicine, or Doctor of Science, or Bachelor of Divinity, or "Bachelor of Laws, or Bachelor of Medicine and Master in "Surgery."

The General Council of the University of St. Andrews has determined to memorialise the Government against the proposed admission of the Medical Graduates.

Under these circumstances it was resolved at the General Session of the St. Andrews Medical Graduates' Association, held on April 8th, 1868, that the claim of the Doctors of Medicine of St. Andrews to a seat in the General Council should be brought to the notice of the governing bodies of the University, and this Memorandum has accordingly been drawn up for that purpose.

University Degrees are of two kinds, Honorary and Ordinary. The broad distinction between them being, that holders of the one have complied with the regulations of the University as to the course of study, and have submitted to an examination; possessors of the other have done neither, but have had their Degree conferred on them by the University as a mark of honour and a recognition of high position or learning.

Honorary Members, as a rule, have no voice in the management of the University, and, when it returns a Member to Parliament, no vote for his election.

Ordinary Members, on the other hand, who hold the Degree of Master of Arts, or one equal to, or higher than it, and keep their names on the register, are Members of the General Council or Convocation or Congregation or Senate of the University, as the case may be, and have a vote for the Member in Parliament. In some Universities the Bachelors of the different Faculties are equally entitled to these privileges with the Masters.

The Doctors of Medicine of the Scottish Universities constitute an exception to this rule. They are excluded from the General Council unless they have given regular attendance on the course of study in the University of which they are Graduates for four complete sessions, or three sessions in that and one session in some other Scottish University.

The Doctors of Medicine of St. Andrews are Ordinary, not Honorary Members, for they have pursued the course of study required by the University, and have passed the requisite examinations.

The General Council of the University of St. Andrews consists at present of 368 Members, only two of them being Graduates in Medicine of the University.

There are at least 1300 Doctors of Medicine of St. Andrews residing in the United Kingdom, and employed in Her Majesty's Services.

St. Andrews may thus fairly be called a Medical University chiefly; for its Medical Graduates out-number the Graduates in both the other Faculties, in the proportion of three to one, and have contributed in a still larger proportion to its revenues.*

But although a large majority of its Graduates are Doctors of Medicine they have, as has been seen, no voice in its General Council.

The chief argument raised against the proposal of the Government to give equal rights and privileges to the Graduates of the different Faculties, appears to be, that the Doctors of Medicine have not resided at, and kept terms in the University.

Residence within its walls and attendance on lectures delivered by its Professors cannot be considered a necessary condition of the admission of Doctors of Medicine to the General Council, Senate, Congregation, or Convocation of a University, with a vote for its Member in Parliament; for the University of London, although it offers no teaching and requires no residence, providing merely a curriculum of study to be observed, and recognising a large number of Medical Schools where candidates for its Degrees may obtain their knowledge, yet is to return a Member to Parliament, and its Medical Graduates are to have a vote in his election.

The Regulations of the University of St. Andrews are essentially the same in character. There is a curriculum of the course of study to be pursued by resident Students in the Faculty of Medicine, similar to that required by the other Scottish Universities where there is a Medical School. But as St. Andrews makes no provision for a complete Medical education within its walls, and as it desires

* See page 247.

learned and well-informed men for its Graduates, it does not demand of them an impossibility, but it accepts the possession of a diploma from such Medical or Surgical Corporations as are entitled to be registered under the Medical Act, as evidence of an extended course of study; and it secures the possession of high attainments by the test of a strict and a stringent examination.

Before 1863 the number of Medical Graduates was only limited by their compliance with these regulations, and their ability to pass the requisite examinations; but since that time, the University has only been allowed to confer ten Degeees annually in such manner, and these on candidates above forty years of age.

Non-residence then is not a fault or defect on the part of the Doctors of Medicine, it is a necessity of the University if it is to grant Medical Degrees at all. It would be beside the purpose of the present statement to enter on a discussion of the advantages which accrue to the Public and the Profession by a regulation which allows men a free selection of their place of study, which permits them to offer themselves as candidates for the higher Degree after they have been some time in practice, and which makes the actual possession of knowledge and not the time and place of its attainment, the test of a candidate's fitness for the Degree of Doctor of Medicine.

Seeing then that the Doctors of Medicine of St. Andrews are so numerous, and have complied with all the regulations of the University, their long-continued exclusion from the General Council must be considered unjust and unwise. But the injustice becomes much more evident now that St. Andrews in conjunction with the University of Edinburgh is about to be represented in Parliament. The Memorandum* submitted in February last to the Chancellor of the Exchequer set forth the claims of the Doctors of Medicine to the franchise, and the justice of the demand is now abundantly acknowledged. It could not be otherwise. It could not be seriously maintained that any body of educated men is fairly represented by the votes of one-fourth of the number of its members. But the same reasons, which show the right of the Medical Graduates to a vote for the Member of Parliament, are no less convincing proofs of their title to a seat in the General Council of their University; for it is only as Members of the University that they can claim the vote; and if they be members of the University, they cannot justly be excluded from the General Council, the common ground on which all Graduates should meet and from which all legislative action should arise.

Springing from the argument based on want of residence, is the plea of want of interest on the part of the Medical Graduates in the welfare of the University. This needs little reply. Up to the present time the Doctors of Medicine have had no chance of evincing any interest in the University, for they have been unnoticed and unrecognised by it. But the rapid growth of the St. Andrews

* See page 237.

Medical Graduates' Association which already, before it is a year old, numbers upwards of five hundred members, and whose professed object is to obtain for the Doctors of Medicine a seat in the General Council of the University and a vote for its representative in Parliament, is sufficient answer to the charge. The Medical Graduates are not, and cannot be, regardless of the interests of their University. Their reputation, and its, stand and fall together. With the credit of the University falls the credit of its Degree, and the rank which it confers on its holder.

Again, the admission of the Doctors of Medicine to the General Council has been opposed in the University itself on the ground that the number of the Medical Graduates is so great that the other Faculties would be overwhelmed by them. If the Doctors of Medicine have a just claim to be Members of the General Council their number can but add to the injustice of their exclusion; and should be no bar to the exercise of the privileges which rightfully attach to every member of a University. But it must be recollected that the Medical Graduates are nearly all men in active practice who could not in large numbers leave their daily duties to attend the meetings of the General Council at St. Andrews. There have, indeed, been times when even two or three voices raised in that Council in defence of the University, as they would have been raised had the Medical Graduates possessed a voice in its deliberations, would have been of vital importance to its interests; as when an attempt was made to exclude it from representation in the Medical Council, and when its Degrees were unrecognised by the Poor-Law Board. There are no conceivable subjects which can ever range the Doctors of Medicine in a body against the Graduates in other Faculties; they have no religious or political party ends to serve; their object is to maintain and, if possible, to enhance the reputation of the Ancient Corporation of which they are Members; and they will never be aught else than an aid and a help to those of their fellow-Graduates in other Faculties, whose object is like theirs the good of Alma Mater.

The Doctors of Medicine of St. Andrews, then, claim the right of becoming Members of the General Council of their University, because they are the most numerous of the Graduates and should not be excluded from a voice in its Government; they claim it because they are really Members of the University, having complied with all its regulations, pursued the course of study prescribed for them, submitted to its examinations, and had its Degree publicly conferred on them; they claim it because their interest in the welfare, their stake in the prosperity of the University is great and vital, for their social and professional status depends, in the first instance, on the character of the Degree they hold, and the reputation of the University from which it is obtained; and they claim it lastly on the ground of simple justice, that they may have equal rights and equal privileges with their fellow-Graduates.

The following is the number of Graduates in Medicine and Arts in each year, from 1836 to 1866, inclusive:—

	Graduates in Medicine.	Graduates in Arts.		Graduates in Medicine.	Graduates in Arts.
1836	11	1		592	94
1837	14	5	1852	65	8
1838	23	3	1853	68	12
1839	28	4	1854	65	12
1840	25	8	1855	53	8
1841	21	7	1856	84	10
1842	35	8	1857	68	11
1843	38	4	1858	73	13
1844	45	6	1859	93	9
1845	106	6	1860	83	7
1846	51	7	1861	107	8
1847	39	9	1862	606	2
1848	22	4	1863	10	8
1849	30	9	1864	10	12
1850	56	10	1865	6	15
1851	48	3	1866	10	13
	592	94	Total	1993	242

Fees paid by the Graduates in Medicine, from 1836 to 1866 for Diplomas .....£53,261 : 5 : 0

Of which there was—

Retained by the University ...£33,331 : 5 : 0

Paid to Government for Stamp £19,930 : 0 : 0

Fees paid by the Graduates in Arts during the same period for Diplomas .....£762 : 6 : 0

No Stamp required.

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